

Zhores Medvedev and the Reputation of Russian Science

By all appearances, the Soviet government is unable entirely to shake off its old Stalinist heritage. A week ago, with the apparently willing collaboration of the Soviet government, and certainly of the Academy of Sciences in Moscow, the city of Kiev was host to the International Congress on Gerontology. No doubt the meeting itself was as much of a success as could be expected—some would have complained that the programme was at once too empty and too crowded, others would have been glad to have picked up in corridors information that would have been otherwise hard to come by. On the face of things, the congress appears to have been no more and no less valuable than other occasions of the kind. Yet it is now clear that the meeting was an opportunity for the authorities once more to assert their will against Dr Zhores Medvedev—a Russian citizen and resident whose patriotism has expressed itself in recent years in a demand that Russian society should be at once more liberal and more humane. Dr Medvedev was due to deliver a paper at the conference. It is now apparent that half an hour or so before the proceedings were due to open, he was carted off to the local police station, faced with trumped-up charges analogous to those known in Britain as a “breach of the peace” and “resisting arrest” and finally shipped back home by train by way of Moscow with a warning that if he chose to return to Kiev, so as to attend the symposia and sectional meetings which had been planned, “other complications were possible”.

Zhores Medvedev is his own best biographer. In the past few years, he has given a detailed account of how he was prevented from attending the International Congress on Gerontology at Sheffield in 1967 (*The Medvedev Papers*, Macmillan, 1971) by bureaucratic means. Quite soon afterwards, he lost his job at Obninsk and a year ago he was for a few weeks confined in a psychiatric hospital on the suspicion of madness, but even that shameful episode has been well documented not merely in a stream of letters from the Soviet Union—a postcard arrived at *Nature* within a week of his incarceration with the message not to worry much—but also in a detailed account of the proceedings which he and his brother, Roy Medvedev, have since published (*A Question of Madness*, Macmillan, 1972). The best account of what happened in Kiev last week is now contained in a letter to Professor L. Hayflick of Stanford University Medical School, the president of the International Congress on Gerontology.

Medvedev writes, no doubt with his tongue in his cheek, to explain his disappearance from Kiev in case this should “induce some alarm among my friends”. He hopes that no attention will be paid to his own plight for the “scientific goal is more important than personal trouble . . . , self-pity and self-guard”. By Medvedev’s account, “my pre-Congress kidnappers” turned up half an hour before the Congress was due to open and treated him “reasonably well and neither searched me nor made other harm”. During the interrogation at the police

station, it became apparent that Professor D. F. Chebodarev, the Russian co-president, was aware that it had been planned that Medvedev’s attendance should be prevented. Although the interrogation was “at first rather rough”, when it became clear that the charge of a breach of the peace based on the allegation that Medvedev’s attempt to enter the congress was illegal was overturned by his possession of a personal invitation to give a lecture, and when it proved hard (in spite of the evidence of two bystanders) to sustain the charge of resisting arrest, Medvedev says that his fears that he would be imprisoned for between ten and fifteen days evaporated. Moreover, he appears to have succeeded in fending off the accusation of forgery and misrepresentation which was later levelled at him by pointing out that admission tickets to the congress were transferable from one person (a friend of his) to another (himself). So, Medvedev continues, “I was kindly offered a car for the delivering to the station, my three new friends [the police] talked about ageing and gerontology . . . and therefore I cannot complain that I left Kiev without talks on the ageing problem. . . .” There follows Medvedev’s sad account of how much he would like to have heard what happened at the congress and an expression of his hope that his friends would not be too much alarmed.

This shabby treatment of Medvedev raises important issues for the whole scientific community. It is true that the Russian authorities have never fully accepted the principle that the scientific community is a community of individuals. Rather, they take the view that scientific research is a corporate and also national endeavour, from which it seems to follow that participation in scientific meetings must be decided not by the individuals concerned but by their superiors. This, no doubt, is why Professor John Ziman (see page 115) has reason to complain that Russians invited to participate in conferences in the west are often replaced by other less appropriate visitors. Even on this narrow view, however, there can have been no justification for preventing Medvedev from using his vacation to attend the international congress in Kiev. Plainly, the Russian authorities are bent on harassing Medvedev. A decade ago, this awkward customer would no doubt have been dealt with much more harshly, but it remains intolerable that a scientist should have been prevented by the intervention of the police from participating at an international gathering of scientists. The Russian authorities must recognize that their treatment of Medvedev will put in hazard the willingness of scientific societies everywhere to collaborate in the holding of international conferences in the Soviet Union. In the past few years, the Russian authorities have been eager to encourage the use of Moscow and other Russian cities for international scientific conferences and their hospitality has been widely welcomed not merely for its generosity but because it has helped to bring Russian scientists more closely into the inter-



national community. But these benefits will count for nothing if attendance at these meetings is subject to political restraints. Scientific societies in the west should now seriously consider whether they should continue to participate in plans for holding conferences in the Soviet Union without formal assurances that the Russian police will not, in their own special way, help in choosing the participants.

The handling of Medvedev at Kiev is also a reminder that the time has come for liberalizing the cultural agreements between the Soviet Union and other governments in the west. A month ago, President Nixon returned from Moscow with a cheerful prediction of a more vigorous programme of collaboration with the Russians in science and technology, but the details are still to be negotiated. The occasion should be one for emphasizing that the Soviet authorities must now relax their old-fashioned restrictions on the freedom of Russian scientists to travel abroad. Professor Ziman is right to insist that it should not be for the Soviet authorities to decide which Russian scientists should accept invitations to conferences in the west. Obviously there are more serious difficulties in the choice of those who are to make extended visits outside the Soviet Union but even here it seems quite clear that the Russians are unreasonably afraid that a visit abroad will be an opportunity for defection. Medvedev himself is an important case. From all that he has written, it is plain that he is a fierce patriot courageously battling for a more liberal intellectual life in Russia. There is, of course, no reason why reform in this direction should not be consistent with the economic system to which the Russians are attached. The time has come when scientific academies in the west must take up the cudgels for Medvedev and for science.

European Shuttle

EUROPEAN governments seem bent on perpetuating their muddle about space. The meeting of the European Space Conference due to have been held this week in Brussels has at the last minute been postponed until September, most probably because there is not yet a common policy on the proposal first made two years ago that European governments should be invited to collaborate with NASA on the development of the notorious space shuttle. At the same time, there is continuing uncertainty about the future of some of the projects on which European governments are already trying to collaborate. The European Launcher Development Organisation (ELDO) is in trouble because of the repeated failure of the Europa 2 rocket, and is now being reorganized. It remains to be seen whether the more ambitious plan to develop a larger rocket-launching system called Europa 3, on which the government of France is keen, can succeed technically, let alone commercially. It follows that enthusiasm for an independent European effort in telecommunications satellites is premature. And in the circumstances it is no surprise that NASA itself has begun to lose interest in the prospects of collaboration with European governments.

No tears should be shed over the prospect that further dithering may mean that the invitation to collaborate on the shuttle is withdrawn. Although the past few

months have seen endless arguments in the United States about the economic benefits of the shuttle, which could, once \$15,000 million has been spent, reduce the cost of putting massive satellites into orbit, it seems usually to have been forgotten that one of the chief arguments for the shuttle is military and strategic. In the past few years, it has become apparent that the US Department of Defense would need to replace the Titan III launcher which is at present the workhorse of the American surveillance satellite programme by a more powerful launcher by the end of the 1970s. By all accounts, it is now planned that military satellites of several kinds should be launched into orbits which reach far outside the synchronous orbit inhabited by civil communications satellites. The shuttle, if and when it works, will be a splendid launching platform for devices such as these. But if NASA had not set its mind on the shuttle, the United States Administration would have found itself having to agree that a roughly similar sum should be spent on a replacement for the Titan III. This no doubt explains why discussions between European governments and NASA have so far been most concerned with the ways in which European countries might contribute to the cost of the project as a whole but to the technology of only some of the peripheral equipment. From a European point of view, however, this is at best a bad bargain. Moreover, if it is doubtful whether the United States will have a sufficient need for launching civil satellites to justify development of the shuttle, it is hard to think that Europe in the 1980s would be able to justify on economic terms its tenth share of the total cost. One way out of the European muddle would be a joint decision that the shuttle should stay American.

Similar arguments should persuade European governments that Europa 3 is not at this stage likely to justify its cost. The calculations would no doubt be more cheerful if the disastrous experience with Europa 2 had been less discouraging but, in any case, there must be serious doubts about the validity of the argument that Europe will remain permanently in the thrall of the United States unless it can develop an independent rocket launcher of its own. The truth is that so far the chief and only profitable application of satellite technology is in telecommunications, and, in this field, there does exist a mechanism by means of which European governments and telecommunications carriers can indirectly make use of American technology. The international consortium called Intelsat employs the American corporation called Comsat as its technical manager, and Comsat has an agreement with NASA on the provision of launch facilities. There is no reason why Intelsat should not be made a more efficient organization than it is. Moreover, there is no reason, given the diplomatic pressure which the members of Intelsat could exert in the United States, why the terms on which NASA provides launching facilities for telecommunications satellites should not be negotiated directly with the international consortium, not simply with its manager. And if that were done, there is every hope that the cost of space technology to European governments would be much less than the cost of developing an independent launching facility.

What should be done with the European institutions for space research which have been nurtured in the past few years? If European governments could agree among themselves that for the 1970s at least they will spend

their energy on modest developments in the technology of launching satellites—making Europa 2 a usable rocket, for example—then it would be possible (and sensible) to force a shotgun marriage between ELDO and the European Space Research Organisation (ESRO). At the same time, it should be possible to use these organizations as means of channelling funds to projects for the development of experimental equipment that could yield important scientific benefits and at the same time help to create a sense of confidence in this new technology within Europe. Here again, it is hard to think that launching facilities would be withheld. Most probably, the United States would be as willing (for a fee) to help as it has been in the past. And if it should come to the worst, there are always the Russians who might be asked to help.

Research Organization

WHILE the House of Commons Select Committee on Science and Technology has been banging again at its favourite drum (see page 64)—the establishment of a Minister for Science and Technology within the structure of the British government—the OECD has produced the first of three reports (*The Research System*, Vol. 1, obtainable from HMSO, £2.45), a sensitive comparison of the ways in which science research is organized in France, Germany and the United Kingdom. The study owes its existence to OECD's brooding about what was called in the mid-1960s the "technology gap". Its chief interest is its patient description of how varied are the ways in which three closely comparable nations set out to meet similar objectives, and its demonstration of the great diversity of the institutions which have come into being in the process. The fact that the study has been carried out at a time when the resources available for scientific research have ceased to grow and have even begun to decline (by the yardstick of the Gross National Product) makes it especially valuable as a means of anticipating how the current strains within the system should be relieved.

For most governments, but especially for the three covered by the OECD study, the chief objective of current policy is to win more economic or social benefits from the sums being spent on research and development, estimated at 2.0 per cent of the GNP in Germany, 2.2 per cent of the GNP in France and 2.4 per cent of the GNP in the United Kingdom in 1970. The growth of the past two decades has in all three countries outstripped the traditional capacity of the universities to be the chief instruments of fundamental research, with the result that specialised laboratories have sprung up throughout Northern Europe. The OECD team is right to draw attention to the way in which the existence of these new institutions has tended, paradoxically, to reduce the mobility of professional scientists and engineers engaged in fundamental research. And in spite of the efforts which have been made to increase the economic spin-off from applied research, the comparative survey concludes that none of the three countries dealt with has hit on an ideal solution. The underlying theme is that in spite of all the extra funds which have been devoted to research and development of all kinds,

Europe so far derives less benefit than does the United States from similar work.

Why should there be these differences and what should be done to remove them? There is a limit to the amount of improvement that might be looked for from further structural reorganization. The fact that all three European nations covered by the study have been equally disappointed by their investment in research and development in spite of having different methods of administration is by itself a proof that the ways in which government organizations are created are not strictly relevant. Structural differences between Europe and the United States of a more organic character may be more important. Where fundamental research is concerned, there are good reasons to suspect that clever people are much more easily moved between universities, research institutions and industry than in the United States. In applied research, European practice differs from American in that a smaller proportion of European expenditure by governments on applied research finds its way into the laboratories of commercial organizations before it is spent. One immediate result is that the link between applied research and commercial exploitation is much more tenuous in Europe than in the United States. And there are fewer strictly commercial enterprises in Europe which are as willing as successful American companies to regard research and development as a potentially beneficial investment. The OECD promises that the third part of the present study will be a comparison between the United States, Canada and Japan on the one hand and Europe on the other and these, it is to be hoped, are two of the crucial issues it will consider.

100 Years Ago



The Names Cambrian and Silurian in Geology

WILL you allow me to express, as an humble worker among the rocks of North Wales, my sense of the high value of the contributions to your pages recently by Prof. Sterry Hunt on the "History of the names Cambrian and Silurian in Geology?" I have long felt—and have not hesitated to express my feeling—that a great wrong was done to Prof. Sedgwick when the North Wales groups of rocks from the Bala Beds to the Lingula Flags—the order of which he was the first to unravel in that difficult region—were unceremoniously engulfed in Siluria.

It has also appeared to me one of the greatest anomalies in English geological classification, that the magnificent and well-defined groups of North Wales should be typified by their attenuated and broken easterly outcrops in the Silurian district of South Shropshire. What Llandeilo section of Siluria is there that worthily represents the Arenig and lower Bala rocks immediately east or west of the Berwyn Mountains? What Caradoc section of Siluria is there at all worthy of the fine series of the Upper Bala rocks of Glyn Ceiriog? Of the unworthiness of the schists near the Stiper stones to represent the Lingula beds of North Wales, Prof. Hunt justly speaks in his papers.

I am glad that justice seems at last likely to be done to the veteran, Prof. Sedgwick, than whom a more philosophical geologist I am persuaded does not exist. Let but a sufficient number of scientific men resolve to use his older and truer, because more natural classification, and the justice will soon be complete.

D. C. DAVIES

OLD WORLD

Davies Under Fire

IN spite of adverse comment during the past few months, the Select Committee on Science and Technology still considers that there should be a Minister for Research and Development in Britain. The committee now repeats its often stated view that Britain has no national policy on research and development—although it considers one imperative with entry to the European Community so near—and that a minister would provide “the strong, independent scientific voice in formulating policy decisions at a high level”.

The latest round in the controversy between the select committee and the government is included in a report published yesterday (*Research and Development Policy*, HMSO, £0.18) and is based on evidence taken from Mr John Davies, Secretary of State for Trade and Industry, and Mr Michael Heseltine, Minister for Aerospace. Evidence was taken soon after the committee published its controversial first report where the appointment of a minister for research and development was suggested and at a time when Mr Heseltine had barely warmed his ministerial chair.

Mr John Davies was far from enthusiastic about the committee's proposals, and the committee now hits back. The committee states that comment on the proposals has “been largely negative” and it repeats its position that unless there is one minister responsible to parliament, who would make recommendations on “the range, structure and balance of the government's research and development as a whole, there can be no proper scrutiny of national objectives and how they should be defined”.

Whether the select committee's final plea will have any effect will soon be apparent, for the government after many delays is now saying that its white paper on research and development will be published within a few weeks. It is, however, doubtful whether the committee's latest broadside would have made any difference if it had been published sooner.

The committee also considers that there is an impending “lack of balance” in the budget of the Department of Trade and Industry, for projects such as Concorde, the RB211, the reactor programme and also the financing of government laboratories are all included in one budget with the first three items accounting for £137.8 million out of a total of £201.1 million in 1971–72. The committee states that such an arrange-

ment strengthens the case for an independent research establishment authority which it proposed in a previous report (see *Nature*, 238, 4; 1972).

The select committee pulls no punches in attacking Mr Davies for saying that a company's attitude towards its research and development is fundamentally different from that of a government. Mr Davies, says the committee, “has confused two things”. While the committee admits that the government has different criteria in making decisions on research and development, it insists that both government and industry have to decide on priorities. Such decisions, according to the committee, “inevitably involve the exercise of some central coordinating authority”. The committee then delivers the punch line—“in industry, this exists, but in government today it does not”.

POPULATION

Declining Fertility

COUPLES married for five years want fewer children than they did at the time of marriage. This trend, revealed last week by Dr John Peel, reader in sociology at the University of York, will no doubt send demographers rushing to alter population projections and will add further uncertainty to the predicted population of Britain in the coming years.

Dr Peel's survey covers 350 married couples in Hull who were first interviewed in 1965, soon after marriage. Then the mean intended family size was 2.61 children. By 1970, and an average of 1.51 children per family later, the intended family size had decreased to 2.23 children per family. This decrease is striking because previous comparative surveys of this kind have consistently shown that intended family size increases in the first five years of marriage, chiefly because of the inci-

dence of unplanned pregnancies. Dr Peel said last week that this reduction of intended family size at Hull, which has already been accompanied by a decrease of actual fertility compared with the expectations of the 350 couples when first interviewed in 1965, cannot be explained simply by the use of contraceptive pills.

In the interval of five years, 106 couples had revised their family intentions downwards and only 31 had increased their intended family size. The decrease which Dr Peel has described appears to be fairly uniform throughout the socio-economic classes but well-to-do people (classes 1 and 2) seem to have made a more drastic reduction of family size than other classes.

The results of the survey are published in the current issue of the *Journal of Biosocial Science* (4, 333; 1972). Fifty-five of the 106 couples who have lowered their sights say that their reasons are economic, while 20 of them were aiming for a smaller family for health reasons. Only six couples had lowered their sights because of the world's population explosion.

Of the 31 couples in the survey who had had to make an upward adjustment in their family sizes, 14 did so because of accidental pregnancies and only 7 said that they had done so in order to have a child of each sex. Two of the couples which had originally decided to remain childless had also in the intervening years decided to become parents.

In view of the decrease in the wanted family size, it is perhaps surprising that the survey showed the same percentage of unwanted pregnancies as had earlier surveys. Additionally, Dr Peel's survey shows that 70 per cent of first pregnancies were planned but for third and subsequent pregnancies, only about one half of the pregnancies are planned. According to Dr Peel, contraceptive failure is not an important contributor to unintended pregnancies.

Aharon Katchalsky Fund

THE Weizmann Institute has formed an Aharon Katchalsky Fund so as to commemorate Professor Aharon Katchalsky, who died in the massacre at Lod Airport on May 30. The intention is that an independent body of trustees should decide how best to spend the funds which become available. Professor Israel Dostrovsky, deputy director of the Weizmann Institute, said last week that it might be possible to commemorate Katchalsky's work not merely by means of fellowships tenable at the Weizmann Institute but also by the endowment of a chair in some branch of science.

Since the publication in *Nature* on June 9 of a letter by a group of distinguished molecular biologists and biophysicists, the Editor has received close on a hundred expressions of support for the concept of an Aharon Katchalsky Memorial Fellowship. The time has now come for translating this encouraging response into monetary terms.

Cheques should be sent to the Aharon Katchalsky Fund either at the Weizmann Institute Foundation, Rex House, 4 Regent Street, London W1, or at the Weizmann Institute, Rehovot, Israel.

ASTRONOMY

Hoyle Stays Home

SIR FRED HOYLE will, after all, remain in Britain. In spite of rumours to the contrary following his decision to resign the Plumian chair of Astronomy and Experimental Philosophy at Cambridge, he will, in early 1973, take up an honorary research professorship in physics and astronomy at the University of Manchester.

The news, which leaked out after a meeting of the senate at Manchester last week, has delighted the astronomy community in Britain and Sir Bernard Lovell in particular. Sir Bernard said earlier this week that even though he was instrumental in persuading Sir Fred to come to Manchester, negotiations Hoyle had announced were started only after his resignation from the Cambridge post.

The University of Manchester has yet to ratify the appointment, which will come up before the council of the university on July 19, but Sir Bernard says that there will "be no problems" and no difficulty in finding the money necessary for Sir Fred to carry on his work. A part of the arrangement is that Sir Fred will spend part of each year in Pasadena, as he has done for the past several years.

What will, perhaps, be as much of a blow to astronomy at Cambridge as Sir Fred's departure is the news that Manchester is going to institute a summer visitor scheme similar to that operated so successfully at the Institute of Theoretical Astronomy during Sir Fred Hoyle's directorship. Sir Bernard Lovell said on Monday that they would go forward with plans to attract eminent scientists to Manchester for short periods and that there would be "no difficulty with money" for this project.

Professor J. C. Willmott, director of the physics laboratories and professor of nuclear physics at the University of Manchester, said on Monday that Sir Fred would divide his time between the astronomy and physics departments. Sir Fred has a great interest in the problems of cosmology which can be experimentally investigated in a nuclear structure laboratory, and Professor Willmott said that his presence will be an impetus to the study of the subject at Manchester. At present, there is no active research in experimental nuclear astrophysics in progress in the department but the facilities, notably a 6 MeV van de Graaff accelerator with associated electronics, are available. Professor Willmott said that he had no immediate plans to restart work in this field but that Sir Fred's presence would be a catalyst for such work and that it was certainly possible that a group would be set up once Sir Fred was installed.

NOISE

No New Scale

UNSATISFACTORY though it is, the noise and number index (NNI) for assessing the nuisance caused by low flying aircraft is unlikely to be revised in the near future. After more than a year of deliberation, the research sub-committee of the Noise Advisory Council has decided that in the absence of anything better the index should remain.

The index was created in 1961 following a survey at Heathrow Airport, London, commissioned by the Wilson Committee on the Problem of Noise. The index is based on the average peak noise level at the ground and the number of flights overhead. The index has been used by the Wilson Committee to calibrate a nuisance scale with the terms "slight", "moderate" and "high".

Convenient though the NNI has been, there have always been doubts about its validity and usefulness. The Wilson committee was well aware of these problems, the chief one being that the basis for determining levels of nuisance is entirely statistical. The results for night time annoyance were inconclusive, and the data on which the index rests come only from Heathrow, so its value at other airports is questionable. The index has nevertheless been widely used in Britain—for example to determine the zoning of building near Gatwick and the siting of London's third airport.

A second, more extensive, survey was commissioned at Heathrow in 1967 to check the 1961 findings, and certain important differences emerged, the consultants recommending that the NNI scale be recalculated and the degrees of nuisance be recalibrated. The Noise Advisory Council decided to examine these recommendations soon after it was set up in 1970.

The chief differences between the two surveys were that in 1967, people seemed less sensitive to the same level of noise as in 1961 and that people were more sensitive to the worst noise level they experienced, rather than the average level, which is the basis of the NNI, and that the level of nuisance drops at night.

But the Noise Advisory Council's subcommittee, after examining these differences, argues that there should be no change, and it is understood that the Department of Trade and Industry will accept the committee's point of view.

In defence of its attitude, the subcommittee says that it is not against change, believing that improvements should be brought in as soon as they are proven, but it fears "that the results of excessive haste would be detrimental to the public interest".

No change therefore should be made

in either the day or night time formulation of NNI. What, then, should be done? Re-examine the 1967 statistical evidence is the subcommittee's answer, and try and settle the doubts it has raised.

INSTITUTE OF PHYSICS

Coming Out of the Woods

AFTER a £35,000 deficit last year, the members of the Institute of Physics have agreed to increases of up to 40 per cent in membership fees. At the institute's annual general meeting on July 4 the necessary 75 per cent majority agreed to the increases which should bring the institute a further £28,000 in income in 1973.

But if the increase in membership fees sees the institute out of the wood for the time being, it is not yet in clover. The deficit last year was the worst ever, and although the increase in membership fees will help, balancing the books next year will be no mean feat. Membership fees make up only some 15 per cent of the institute's total income, with publications providing the lion's share. Last year these fared about as well as in 1970, but expansion with an eye to future profit meant that less money was available for use elsewhere in the institute. Dr L. Cohen, secretary of the institute, said this week that "the last two years have been very tough, and there is little sign of a let up in the next year or two".

The officials of the institute itself saw the problems coming some time ago, but the membership was less far sighted. In 1971, two attempts to raise subscriptions, first by 50 per cent and then by 25 per cent, both failed to get the necessary 75 per cent majority, and the institute's staff was very relieved to see the increases go through this time.

For the future, the institute hopes to see its publications prosper and it hopes to cut its costs to keep on an even keel, but with inflation continually chipping away at the value of its fixed income, it seems only a matter of time before further measures will be necessary.

SOVIET SCIENCE

COMECON Meeting

from our Soviet Correspondent

THE meeting of the COMECON council in Moscow, which opened last Monday, represents a new stage in the technological and scientific integration of the Eastern European bloc.

So far, this integration has taken the form of specific projects; *Druzba* oil pipeline, the Interkosmos series of

satellites (the seventh of which, carrying experiments from the Soviet Union, Czechoslovakia and East Germany, was launched on June 30), and the multinational steel complex being built in the Kursk-Belgorod region of the Urals. Over the past few months, however, a new trend has become apparent—the creation of specific organizations to deal with wide-ranging problems of interest to the entire COMECON bloc.

At the end of February this year for example, an agreement between the six leading COMECON countries resulted in the establishment of "Interatom-instrument", an organization to cover scientific, technological, production and trade cooperation between the member countries in equipment for nuclear research, atomic power engineering, and dosimetric and radiometric devices. The Russian *Ekonomicheskaya Gazeta* (No. 14, 1972) noted that this was the first multilateral international organization of the socialist bloc fully to unite scientific and technological activity with production, and marketing. "Interatom-instrument", in fact, seemed a precursor of a whole new range of COMECON scientific subsidiary organizations.

The Budapest conference of June 1972, attended by representatives of Bulgaria, Hungary, East Germany, Cuba, Mongolia, Poland, Rumania, Czechoslovakia and the Soviet Union, discussed cooperation in space research. The Interkosmos programme was discussed, together with future plans for joint research in space physics, space communications (the proposed "Inter-sputnik" programme) and the biological, medical and meteorological possibilities of future space research.

The general secretary of COMECON, Nikolai Fadeev, writing in *Sotsialisticheskaya Industriya* (July 3, 1972), stressed that the International COMECON Investment bank is operating satisfactorily and that, for example, the credits it granted in 1971 represented an increase of some 40 per cent compared with 1970. The total transfer of funds between the member countries (some 40,000 roubles) was 10 per cent up on 1970. Full financial and economic integration of COMECON, begun in 1971, will take 15–20 years to complete. In the meantime, while carrying out its large-scale projects for the gradual economic unification of the socialist bloc, COMECON faces financial difficulties. Its chief aim, according to the agenda of the Moscow conference, is further economic and monetary integration—which on the technological front is represented by increased exploitation of Soviet raw materials and their distribution to the countries of the COMECON bloc.

The demand for raw materials is a pressing problem and Hungary, in

particular, has for some time been stressing the need for long term commitments on the part of the Soviet Union. To exploit the Soviet raw material resources, however, financial commitments by the COMECON bloc are demanded. It is rumoured that not only will Russia continue to move millions of her population to the developing areas of Siberia—the source of this potential wealth—but that she will also increasingly call on her COMECON partners to supply the necessary equipment, technicians and experts, irrespec-

tive of the home commitments of the countries concerned.

In *Sotsialisticheskaya Industriya*, Fadeev speaks of certain countries seeking cooperation with COMECON. The agenda of the Moscow conference, and the unofficial reports circulating in conjunction with it, would suggest that this "cooperation" should be interpreted as the need, at present, for Western capital, to support the technological developments upon which the new prosperity of the Eastern bloc is to be founded.

Prospects for Reykjavic

During the present World Chess Championship in Iceland, Nature will publish a weekly commentary on the games by Dr Jonathan Penrose, British Chess Champion for 1958–63 and 1966–69. His first article appears below.

The chess match between Boris Spassky (the reigning World Champion) and Robert Fischer (the official challenger) taking place in Iceland has fired the imagination of the public in a way that no chess match has done before. This is probably because for the first time since the war there seems to be a real threat to the dominance of the World Championship by Russian players. A further contributing factor has obviously been the unusual behaviour of the American challenger, which has led not only to a big increase in the stakes for the prize money but also to an embarrassing delay in the start of the match.

As a player, however, Fischer shows the hallmark of genius. His opening play is generally well prepared and deep, his middle-game play logical and forceful, his end-game play persistent and accurate. In addition he has an amazingly quick sight of the board and generally plays his moves at a speed well within the allotted time allowed for tournament or match games (forty moves to be completed by each player within 2½ hours). He is a superb "lightning chess" player and won a very strong tournament held for this form of chess in Yugoslavia in 1970 ahead of most of the strongest players in the world (although Spassky was not competing). His general tournament record over the past few years has also been very impressive, and his rise to the position of challenger has been achieved by some phenomenal match victories over such players as Taimanov, Larsen and Petrosyan. One of his distinctive characteristics

until now has been his intense desire to win his games at all costs.

Fischer's weaknesses probably stem from impulsiveness and over-confidence at times. This has occasionally resulted in unexpected defeats by players normally considered much weaker than him. Also, he has been on such a wave of success over the past few years that it is difficult to predict what his mental attitude would be if he were to find himself two or more games down in the present match.

In contrast to Fischer, Spassky's rise to the chess heights has been much less spectacular. During the 1960s, Spassky's development was continuous but slow. He has managed to temper a highly imaginative gift for the game with a fine degree of solidarity and tenacity that gives the impression of the complete professional player. His style is comparable to Fischer's (although his opening repertoire is more varied) and his successes in the "candidates" matches during the 1960s were almost as impressive as Fischer's recent run of victories.

Since winning the World Championship against Petrosyan in 1969, however, Spassky has shown a tendency to relax in important tournament games and has allowed many weaker opponents to escape with peaceful draws. Nevertheless, a possible signpost to the outcome of the present match may be found in the complicated game that Spassky played with Fischer at the Olympiad in 1970 when Spassky pulled out all the stops to win. Also, his record in individual games with Fischer is good. It may be that Spassky needs to be goaded to produce his best form, and if the match takes its course reasonably smoothly from now on, there is every indication that the quality of the chess could be tense and uncompromising.

JONATHAN PENROSE

NEW WORLD

DES—Another Black Sheep?

by our Washington Correspondent

A BITTER dispute is brewing about the merits of using a synthetic hormone, diethylstilboestrol (DES), for fattening sheep and cattle. In the debate so far, well defined commercial benefits have been staked against possible but unquantifiable health hazards. But both sides have also contributed the usual quota of speculation and disputes about the significance of facts.

The debate should come to a head in the next few months, for the Food and Drug Administration has now announced public hearings on the issue of whether DES should be banned from animal feeds. A lawsuit filed last November by the Natural Resources Defense Council, charging that DES should be banned immediately (and, indeed, that it should never have been allowed on the market in the first place), will also be heard in the federal courts. DES seems destined to take the place of DDT as the black sheep of American agriculture.

The legal issue is remarkably simple—at least, in theory—for DES is a carcinogen. Several animal feeding studies have shown it to be an extremely potent cancer agent and last year DES was linked with a number of cases of a very rare cancer of the vagina in young women. The unambiguous clause in the food and drug laws known as the Delaney Amendment forbids the use in food of any additive that has been found to cause cancer in animals. The law therefore prescribes that DES must not get into the food supply—no ifs and buts, no cost-benefit analysis.

The Food and Drug Administration has been trying to ensure that DES does not get into food. In October last year, it announced that sheep and cattle should be taken off DES-treated feed for seven days before slaughtering. The regulation was intended to ensure that all hormone would be excreted and that none would remain in the tissues. Shortly afterwards, Dr Charles C. Edwards, Commissioner of the Food and Drug Administration, said that “should these measures prove unsuccessful (in eliminating DES from the food supply), we are prepared to ban its use entirely in animal feeds”.

Unfortunately, the real world is not that simple. The test for the presence of DES in animal tissue can pick up only residues greater than 2 parts per thousand million, a concentration held by some scientists to be too high for such a powerful carcinogen. Others

argue, however, that the risks from concentrations of DES below the level at which it can be detected are minimal or non-existent, and that the risk is outweighed by the economic benefits derived from using the hormone to fatten cattle. Cost-benefit analysis has therefore crept into the debate.

But even with present detection methods and in spite of the more stringent regulations applied last year, DES has been found in 1.9 per cent of the animal livers sampled during the first six months of this year. It was this finding that caused the FDA to announce last month that it will hold public hearings to determine whether or not DES should be banned from animal feed. But, in the week after the FDA's announcement, the detection rate shot up to 10 per cent, and in the first two weeks of June, DES was found in 5.1 and 5.4 per cent respectively of the livers analysed.

The FDA's decision has been greeted by some harsh criticism, especially from Mr L. H. Fountain, chairman of the subcommittee in the House of Representatives which has oversight over the affairs of the FDA. Fountain last week called the FDA's proposed action “extraordinary”, especially in view of the fact that Edwards said that he was prepared to ban DES from animal feeds if it continued to show up in meat and other animal tissues. Fountain also said that he believes that the FDA announcement “appears to be merely a tactic for delaying the regulatory action that the law requires”, and finished with the warning that “if the FDA does not act responsibly to enforce the law and enforce it expeditiously in this and other matters, I believe the Congress will have no alternative but to take firm corrective steps to protect the American people”.

Why has the FDA not simply taken DES off the market? The answer lies partly in agricultural economics. Cattle fed with DES reach market weight of 1,000 pounds 35 days sooner, thus saving 500 pounds of feed, and the US Department of Agriculture has estimated that banning the hormone from use in animal feeds would add some \$3.85 to the average annual shopping bill in the United States. “DES is clearly a useful and effective product”, the FDA says. But the FDA also wishes to discover why the regulations forbidding administration of DES seven days before slaughter have not worked. Is DES inherently incapable of being properly regulated, in which case there is no alternative but to ban it, or would

more stringent controls be sufficient?

Radioactive tracer studies have shown that DES is removed from the tissues of cattle within twelve hours of ingestion, and that the liver is its last stronghold. In theory, then, the seven-day withdrawal period should amply ensure that DES from animal feed does not enter man's food supply, so that the high incidence of DES in beef liver would seem to indicate that farmers are simply not obeying the rules. One problem is that a large proportion of beef in the United States is still produced by small farmers who have only one feedlot for their cattle. This means that they must take all of their cattle off DES feed together, which clearly presents problems when not all of the cattle are ready for market at the same time. It was also suggested in a report prepared last year by a task force working under the auspices of Ralph Nader (see *Nature*, **232**, 216; 1972) that farmers frequently sell off their cattle as soon as the market price is right, the whole transaction being completed within a matter of hours so that cattle finish up at the slaughterhouse without having been taken off DES feed.

The FDA's critics are also sceptical of the economic arguments which have been put forward. For one thing, it is argued that the huge grain surplus in the United States, which will not be much affected by this week's agreement to sell \$350 million worth of cereals to the Soviet Union in the next three years, could be used to make up for DES. Others argue that in the meat market, only the more expensive cuts of meat would be affected, while Mr Fountain has raised the question whether consumers would be “willing to risk exposure to a cancer-producing drug for \$3.85 a year”.

Carcinogenicity will be the chief item of discussion at the hearings and the court case, and two events last year have sparked off renewed concern about the drug. The resurrection of the results of a study published in 1964 which suggested that DES is carcinogenic to some strains of mice at levels down to 6.25 parts per thousand million (Gass, G., Coates, D., and Graham, N., *J. Nat. Cancer Inst.*, **33**, 971; 1964), is the first. The second was a piece of scientific detective work by which Dr Arthur L. Herbst of Massachusetts General Hospital tracked down an association between an outbreak of eight cases of a rare adenocarcinoma of the vagina in women aged between 15 and 20 and DES taken by their mothers to prevent

threatened abortion. Herbst's finding seems to have been the first clinical evidence that foetal exposure to a carcinogen can cause cancer, and it also bridged the gap between animal feeding studies and direct observation in man (Herbst, A. L., Ulfelder, H., and Poskanzer, D. C., *New England J. Med.*, **284**, 878; 1971).

Although the two findings are essentially irrelevant to the purposes of the Delaney Amendment, which requires only that DES causes cancer in animals for it to be banned from the food supply, they have cast doubts on the FDA's testing procedure and raised the spectre of risks from amounts of DES in meat and liver which may be below the detection threshold.

The issue is complicated by the fact that DES is no different from other natural and synthetic hormones in its carcinogenicity, and that the small amounts that may be present in meat are negligible compared with the quantities to which people are naturally exposed from their own glands and even plants. At earlier hearings before Mr Fountain's committee, it was suggested that 500 pounds of liver contaminated with DES at the level of 2 parts per thousand million would contain the same amount of oestrogen as that produced by a premenopausal woman in a day.

Others argue, however, that DES is several times more powerful as an oestrogen than oestradiol, the natural hormone, and that it is several thousand times more potent than the most potent plant hormone. A direct comparison between the amount likely to be ingested in meat and the daily production in man is therefore misleading. And where carcinogens are concerned, is not the only safe level none at all? Dr Umberto Saffiotti, Associate Scientific Director for Carcinogenesis at the National Cancer Institute, suggested at Mr Fountain's hearings that there is a strong case for not allowing any chemical carcinogens in food use. "I think it is a very prudent policy which is being widely accepted by people in cancer research."

DRUG ABUSE

Antagonists in Sight

by our Washington Correspondent
It is now nearly five months since Congress passed legislation formally establishing the Special Action Office for Drug Abuse Prevention in the White House, and the office has already made its presence felt in several ways. Its first action was to urge on the recent decision to make methadone more widely available for treating heroin addicts (see *Nature*, **236**, 322; 1972). Now, Dr Jerome Jaffe, Director of the Special Action Office, has announced that the National Institute of Mental

Health is to expand research on narcotic antagonists.

Both policies are logical in the light of Dr Jaffe's own professional background. Jaffe pioneered methadone maintenance in Illinois before joining the Administration and was also responsible for much of the early work on cyclazocine, the most widely studied narcotic antagonist. But the two moves also fit in with past policies for combating the rising tide of drug addiction in the United States—because of the size of the problem and the political and social need to find a quick solution, much effort has been expended on trying to find a technological fix in the shape of drugs which will help an addict to break his habit or even help to avoid addiction. One noticeable benefit has been a loosening of the pursestrings for pharmacological research.

Several directions of research are being opened up. They include attempts to develop a non-addictive analgesic for medical practice, efforts to find long-lasting potent drugs to block the euphoric effects of heroin without causing side effects and—a long shot—an attempt to demonstrate antibodies to heroin which might be used in vaccinations to prevent addiction. Tying these projects together is an extensive body of basic research on the underlying biochemical basis of addiction. Until there is a well established model for addiction at the molecular level, the search for a technological fix will at best be hit and miss.

How is the programme going? It is only fair to say that everybody concerned with drug addiction accepts that drugs by themselves cannot provide the whole of a solution to drug abuse, for psychological factors lead to addiction in the first place. But those who would use drugs to treat addiction argue that drugs have a useful part to play alongside other approaches such as group therapy and psychological counselling, and that they can in some cases be stepping stones to a cure. William E. Bunney, jun., Director of the NIMH Division of Narcotic Addiction and Drug Abuse, said last week that "we are at a point where there is cause for optimism that we can develop effective antagonistic drugs".

This remark came with the announcement that grants totalling more than \$2 million are to be awarded for clinical and pre-clinical testing of narcotic antagonists, and to support a conference on the topic later this year. The clinical research will be chiefly confined to cyclazocine, used on heroin addicts in various stages of withdrawal and in a variety of psychological surroundings. The clinical research will be focused on other possible narcotic antagonists which have shown promise in animal studies. The new programme also represents a considerable expansion of

present basic pre-clinical research on narcotic antagonists, now running at about \$1 million a year.

Antagonists function by sitting on receptor sites in the brain, thereby preventing access to the receptors of opiate molecules. The antagonist will also displace opiate from the receptor, so that if it is administered after an opiate, it will quickly nullify the effects of the drug and produce withdrawal symptoms in an addicted person. If it is given before an opiate, on the other hand, it will prevent effects.

Antagonists have two potential uses. First, administration of an antagonist may help an addict who has recently been withdrawn from heroin to stay drug-free by blocking euphoric effects and thus removing the reinforcement of drug-seeking behaviour. The most promising application of antagonists may be their use to prevent people who have been experimenting with heroin from becoming addicts. Dr Jaffe suggested last week that since rapid and reliable tests to detect heroin use are now available, "some medical or community groups may elect to use such techniques in high risk populations . . . and thereby discover heroin experimenters before they become addicted, involved in crime, or die of overdoses. Once diagnosed, these early potential addicts could be temporarily immunized by daily treatment with antagonists."

The snag is that no available antagonist is ideal for widespread use. To be offered in a drug treatment programme, antagonists would have to be orally administered, potent, have few side effects and produce no tolerance. But cyclazocine, the most widely studied drug, produces headaches, blurred vision, sedation, feelings of depersonalization, and hallucinations. Patients can, however, become tolerant to the side effects if the drug is administered over several days. Another widely studied antagonist, naloxone, produces very few side effects, but its antagonistic effects last for only four to six hours, compared with 24 hours for cyclazocine. But animal studies on a drug with the code name EN-1639 A suggest that it may be three times more potent than cyclazocine, last for a day and produce few side effects. It has not yet been given clinical trials, however.

Whether or not clinical trials with the new antagonists turn up an effective agent, it is clear that the most important task for improving rehabilitation and treatment for heroin addicts is to expand and improve drug addiction clinics. There are signs that Jaffe's office is conscious of the need, and facilities are indeed slowly being improved, but there is clearly need for expansion. The opening of a new clinic is, however, not such a newsworthy event as progress towards the technological fix.

NEWS AND VIEWS

Elementary Particles and Cosmology

ON page 86 of this issue of *Nature*, Hoyle and Narlikar put forward the idea which they have been studying for a decade or so now that local phenomena cannot be understood in isolation from the rest of the universe. Most, though not all, of their previous work in connexion with this hypothesis has been devoted to showing how it predicts and illuminates generally accepted theory. Their work on electrodynamics, for example, has shown how the influence of the rest of the universe, through an interaction whose structure is much simpler and more elegant than conventional local theory, leads nonetheless to the right answer—namely, the same predictions as Maxwell's theory. The distinctive and controversial character of their work lies chiefly, though not entirely, in the conditions on the structure of the universe over large distances and times (in the future as well as in the past) which make such a deduction possible. The importance of the present article lies in the fact that it purports to explain a phenomenon which local theory has always regarded as given and inexplicable. If explanations of this type are successful they will establish not merely that one can use theories which treat the universe as a whole, but that one must do so.

It is a paradoxical feature of modern theoretical physics that the possibility of large scale concerted cosmological interaction should still be controversial. It would be possible to construe the progress of physics from Copernicus to Dirac as the lingering demise of this possibility. Copernician heliocentricity ended mundane cosmological purpose. Newtonian action at a distance which kept the planets in their new courses soon gave way to Laplacian field theory in which the intervening space was filled by fields described by differential equations so that each part acts only on neighbouring points—a change only in mathematical method certainly, and not in hypothesis, but one without which Einsteinian gravitational theory would not have been psychologically possible or plausible. That theory (or mathematical method) was so successful that it was applied to electrodynamic interactions as soon as they were discovered. Maxwell went so far as to assert that electromagnetic fields are the sole reality and that material charges are mere vortices which happen to exist in them. In modern times matter has lost its solidity and given way to space-filling fields. Physics, then, has been increasingly concerned with continuity and local interaction and less with interaction between discrete particles at a distance.

The paradox is that the mediaeval concept of distant interaction should have survived at all. The reason is that, provided the particles in the universe past and future conform to certain conditions which Hoyle and Narlikar believe to be satisfied by the universe, direct interaction provides exactly the same predictions as field theory about the motion of the particles themselves. From this standpoint fields are merely good mathematical methods.

The rotatory bucket experiment suggested by Bishop Berkeley is one very striking piece of evidence in favour of the view that large scale interactions affect local phenomena. The surface of a bucketful of water is flat only when the bucket is not rotating. But not rotating relative

to what? Experimentally the answer is the distant stars. In Newtonian theory one has to posit the existence of non-rotating frames of reference in which Newtonian mechanics works (making the water surface flat) and ascribe to coincidence the non-rotation of distant matter in such a frame. Mach suggested that it is distant matter which causes certain local frames to behave like non-rotating ones. This suggestion is known as Mach's principle. (A full account of it will be found in D. W. Sciama's book, *The Unity of the Universe*, Cambridge University Press, 1966). It was in the hope of incorporating such a causation into a field theory that Einstein proposed the modification of Newton's theory of gravitation which comprises general relativity. Although it is certain that Einstein's theory was an advance on Newton's, both for elegance and for exactitude, it did not succeed in incorporating Mach's principle. It was Hoyle and Narlikar's prescription for expelling fields from physical interaction and replacing them with direct interaction, applied to general relativity, which gave rise to their theory of gravitation and succeeded in incorporating Mach's principle. Hitherto the most interesting feature of their theory has been that it reduces to general relativity unless the range of interaction is either very long or very short. When it is very long, the influence of very distant parts of the universe, and the need to adopt one type of cosmological theory to make it all work, are apparent. When it is very short the differences from general relativity have not been amenable to experiment. Hoyle and Narlikar's present article represents an advance. They show how to combine their ideas about long range interaction in gravity and electrodynamics and their ideas about quantum mechanics. The combination is an unfamiliar-looking mixture of integrals along paths in real space time and spinors which live in an abstract space invented to account for local phenomena manifested by single particles.

Nature has provided, not one, but several charged particles. As well as the familiar electrons and protons there are muons and the corresponding antiparticles. Electrons and muons have in common that they take part only in weak interactions, not in strong ones, and they have identical charges but different masses. It is natural, therefore, to consider them together in a theory which sets out to show how mass is determined.

To this end the theory combining Hoyle-Narlikar gravitation with spinors is generalized still further in the present article. Spinors representing muons, electrons and the neutrinos involved in their interaction are arranged in a certain order in a matrix. Matrices, of course, do not explain anything; a matrix is merely a bookkeeping device for thinking about several quantities at once by arranging them in tabular form. Just as it is usual, however, to represent a relationship between a pair of numbers on a graph and to think of the shape of the graph as a real solid object, so it is usual to think of a matrix as living in a "space". The postulate in the present theory is that this space is isotropic so that transformations which distort it do not affect the theory.

There are not many interactions which satisfy this

requirement. One of them is interpreted as electromagnetism and it shows that neutrinos do not interact electromagnetically and that the other particles have the same charge. Another possibility is gravitational interaction which (according to Hoyle and Narlikar's ideas) allows the mass of each particle to be determined by that of all other particles in the universe. The effect of the new refinement is that now it is possible to obtain a zero mass for the neutrinos without getting any inconsistency and that the mass of each electron is determined by all the muons in the universe and *vice versa*. The electron is lighter because there are fewer muons. The muon is heavier because there are more electrons. Moreover, if the universe had originally had perfect symmetry and equal numbers of each, a decay of one into the other (say muons into electrons) would have made muons slightly less numerous and heavier and perpetrated the same process.

Quantitative agreement is not good. Hoyle and Narlikar do not predict the correct numerical mass ratio, and the authors ascribe this failure to electronic self-interaction. One could predict that the lack of numerical success will be the point seized on by the detractors of whom Hoyle and Narlikar, like all good controversialists, are never short. But the method is the important thing, and their conclusion that large scale features of the universe may be responsible for the constraints of local phenomena is a crucial hypothesis which should prove fruitful in the future.—From a Correspondent.

Glutamate Antagonists

THE report in this week's issue of *Nature New Biology* (238, 61; 1972) by J. Davies and J. C. Watkins that 1-hydroxy-3-aminopyrrolidone-2 (HA-966) is a selective antagonist of the excitant action of L-glutamic acid on cortical neurones makes these workers the fourth group in recent months to declare an interest in glutamate antagonists. Clearly these groups are undaunted by Werman's recent (*Ann. Rev. Physiol.*, 34, 337; 1972) appeal to his colleagues to abandon their studies of transmitter action based on blockers and measurements of neuronal firing rates and to concentrate on measurements of membrane polarization, changes in conductance, reversal potentials and determinations of the ionic processes initiated by putative transmitters.

Unfortunately it is clear from a recent review by Johnston (*Brain Res.*, 37, 1; 1972) and a more recent study by Bernardi *et al.* (*Brain Res.*, 39, 523; 1972) that the role of glutamate as an excitatory transmitter in vertebrates has not been clarified by the various attempts made with intracellular microelectrodes to analyse the response of the postsynaptic membrane to excitation by either synaptic stimulation or iontophoretic applications of glutamate. Although neurones have been shown to be excited and depolarized by glutamate, the evidence does little more than suggest that the region of the membrane excited is also the region influenced by excitatory synapses. On theoretical grounds the excitatory reversal level is believed to be far removed from the resting potential, perhaps far beyond the level that can be attained by the limited current which can be passed through fine intracellular electrodes. Furthermore,

synapses on dendrites may be far removed from electrodes located at their usual site in the cell soma. It is therefore not surprising that the conductance changes and reversal levels measured to date are small and poorly defined. This situation must surely justify the current search for antagonists of either excitation or glutamate action.

Priority for discovering the first example of a specific interaction between glutamate and an antagonist is claimed by Boakes *et al.* (*Brit. J. Pharmacol.*, 40, 202; 1970), who somewhat unexpectedly found the excitatory action of glutamate to be blocked by LSD-25, a compound which surely fulfils Werman's latest dictum for antagonists that a certain mystique must surround their discovery, usage and molecular structure. LSD-25 was a specific blocker in the sense that the action of D,L-homocysteic acid was unaffected. This interaction was, however, restricted to those cells of the brain stem which were excited by 5-hydroxytryptamine. Presumably further studies of LSD-25 elsewhere in the nervous system were discouraged by earlier reports which showed LSD-25 to cause a non-specific reduction in neuronal excitability.

Nearly a year ago, at a satellite symposium of the International Congress of Physiology held in Basle (*Experientia*, 27, 1107; 1971), two more groups named compounds which they believed to be glutamate antagonists. Curtis reported that he and his colleagues had found methionine sulfoximine and 2-methoxyaporphine to possess some of the necessary characteristics. McLennan has now published a full account of the observations which led him to name two glutamic acid derivatives as his favourites—DL- α -methylglutamate and the diethylester of L-glutamic acid (*Brain Res.*, 39, 419; 1972). Iontophoretic ejection of either compound depressed the sensitivity of many thalamic neurones to glutamate ejected simultaneously from the same microelectrode array while leaving the response to DL-homocysteic acid and acetylcholine intact. Unlike many of the other glutamic acid derivatives tested, neither substance caused excitation of the neurones. Furthermore, the excitatory response of some cells of the nucleus ventralis posterolateralis thalamic to electrical stimulation of peripheral nerves was impaired during the loss of glutamate sensitivity. Presumably if more stringent tests confirm the specificity of these compounds for glutamate receptors, the role of glutamate as an excitatory transmitter in the mammalian central nervous system has been established.

This week Davies and Watkins draw attention to HA-966 (1-hydroxy-3-aminopyrrolidone-2), a much more potent depressant of the excitant action of L-glutamate and L-aspartate on neurones. Although HA-966 is inactive on neurones of the cerebral cortex firing spontaneously or excited by acetylcholine, its potency on cells excited by glutamate is only slightly less than that of γ -aminobutyric acid, for depressions of equal magnitude occurred when HA-966 and γ -aminobutyric acid were released alternately with currents which differed by only a factor of two. It is difficult to foresee how a direct action of this compound on either the ion conductance of the resting membrane, the firing threshold or the rate of rise of action potentials can be excluded without the use of intracellular electrodes.—From a Correspondent.

Thermolysin Analysed

from our Molecular Biology Correspondent

THE latest enzyme to be given the full treatment, of sequencing and X-ray structure determination, is thermolysin, a protease from a thermophilic bacterium, which hydrolyses peptide chains on the carboxy-side of hydrophobic residues. It is a metalloenzyme, and contains both zinc and calcium, though only one zinc atom is indispensable for activity. It bears no relation to the familiar serine or thiol proteases, but in terms of molecular weight (which is 35,000), composition, and in particular the involvement of zinc in the catalytic process, it bears at the very least a passing resemblance to the animal enzyme, carboxypeptidase.

The sequence, however, which has been determined by Neurath and his group (Titani *et al.*, *Nature New Biology*, **238**, 35; 1972), belies this similarity, for a comparative analysis reveals no homologies. The only noteworthy feature of the sequence is the considerable clustering of residues of like charge in different parts of the chain. The electron density map at 2.3 Å resolution is shown by Matthews *et al.* (*ibid.*, 41), and reveals a now rather familiar pattern for proteins in this size range, two distinct globular units being separated by a deep groove, within which is lodged the zinc atom; its location was established by extraction with a chelating agent and replacement by mercury. Next to the zinc is a hydrophobic hole, into which a substrate side chain might be expected to fit, and so determine the specificity of the enzyme. A length of α -helix, from which emerge two ligands for the zinc, runs along the inside of the groove, and another zinc ligand comes from another long helix, which traverses the molecule. The two globular elements of the structure are made up of the C-terminal and N-terminal parts of the chain, and the authors draw attention to the relative prevalence of apparently independently folded units of this size—a faint echo perhaps of Svedberg's theory, which at one time had wide currency, that all proteins were made up of subunits of about 17,000 molecular weight.

One of the most cogent questions concerns the ligation state of the

active site zinc atom. The ligands have been identified on the basis of the position of those residues in the sequence which are seen to have side chains approaching the zinc (Matthews *et al.*, *ibid.*), and comprise two histidines and a glutamic acid, disposed in more or less tetrahedral manner. These, as enzymologists will instantly recall, are the very same ligands as surround the zinc atom in carboxypeptidase. Matthews *et al.* observe that such common active site features in otherwise disparate enzymes of related specificity have been encountered before, in particular between the animal serine proteases and the bacterial enzyme, subtilisin. The similarity of the thermolysin to the carboxypeptidase site does not apparently end here, for the cavity behind the zinc atom contains a glutamic acid residue, perhaps corresponding to a similar side chain in carboxypeptidase. Close by, there is also an aspartate arginine ion pair, the arginine, which may again have a counterpart in carboxypeptidase, being apparently hydrogen-bonded to a histidine.

It is not certain whether the essential tyrosine of carboxypeptidase is likewise represented, but there is a tyrosine in the cleft, which may be hydrogen-bonded to one of the zinc

ligands. Just how fundamental the similarity between the two enzymes really is in terms of mechanism is uncertain, because one can only guess at how the substrate fits into its slot. Certainly the overall geometry round the zinc, in spite of the identity of the ligand side chains, is distinctly different. From the reported presence of subsites in the enzyme, which recognize parts of the substrate on either side of the susceptible bond, it must be supposed that the cleft is lined with residues that participate in such interactions.

There remain the calcium binding sites: calcium ions are not, it seems, essential for activity, but reportedly stabilize the enzyme towards the high temperatures, at which the bacterium contrives to flourish. The X-ray structure reveals four calcium ions, two of them close together in a complex site, involving five side chain carboxylate groups, partly buried below the surface. The coordination of the calcium ions is thought to be octahedral. A third calcium ion is bound at the surface, apparently by way of two aspartates, and a fourth has also been identified at another aspartate side chain.

Insulin: in referring recently (*Nature*, **237**, 135; 1972) to work by Zimmerman, Kells and Yip on guinea-pig insulin, I allowed an error to creep in. The statement that, unlike bovine insulin, this protein does not associate "beyond the dimer" should of course read "to the dimer".

DNA Base Composition and Electrophoretic Mobility

IN *Nature New Biology* next Wednesday (July 19), Zeiger, Salomon, Wesley, Dingman and Peacock report a series of experiments of general significance to the many biochemists who separate DNAs by electrophoresis in polyacrylamide gels. Zeiger and his colleagues have obtained convincing evidence that the electrophoretic mobility of double stranded DNA is dependent not only on the size of the molecule but also on its base composition.

The first hint that this might be the case came when the group found that they could efficiently separate mouse satellite DNA from mouse main band DNA by polyacrylamide gel electrophoresis. The traditional way of achieving this separation is, of course, to centrifuge sheared mouse DNA and rely on differences in buoyant density, arising from differences in base composition, to effect the separation of A-T rich satellite DNA from G-C rich main band DNA.

Pursuing this lead Zeiger and his colleagues successfully attempted to separate by gel electrophoresis crab satellite DNA, which is almost pure poly dA-dT, from the main band DNA. Both mouse and crab satellite DNAs migrate more slowly than the main band DNA and their separation from the main band DNA must in part result from differences in base composition because molecules in the two separated bands show only small differences in molecular weight. These differences are not great enough to account for the electrophoretic separation.

Experiments with sheared and un-sheared DNA from bacteria confirm this conclusion; for example, DNA extracted without shearing from three species of bacteria, *Bacillus subtilis* (42 per cent G+C), *Aerobacter aerogenes* (56 per cent G+C), and *Pseudomonas fluorescens* (63 per cent G+C), have electrophoretic mobilities respectively of 3.32, 3.53 and 3.66.

DNA COLIPHAGES

Another Unwindase

from our Cell Biology Correspondent

T4 GENE 32 protein, the Alberts protein, T4 unwindase, call it what you will, is one of the more celebrated of coliphage proteins. It is, of course, as Alberts and his associates have elegantly demonstrated, the protein, specified by gene 32 of phage T4, which apparently unwinds the double stranded T4 DNA and aligns the two parental, template strands in advance of DNA polymerase replicating the molecule.

Since the discovery of T4 gene 32 protein, other proteins with similar properties *in vitro* have been identified in organisms as diverse as *Escherichia coli* and calf thymus cells, and it is beginning to look as though cooperatively binding proteins capable of denaturing double stranded DNA are a universal prerequisite of DNA replication. It is not perhaps surprising, therefore, that Alberts, Frey and Delius and Oey and Knippers can now report (*J. Mol. Biol.*, **68**, 125 and 139; 1972) that the synthesis of progeny, single stranded circular genomes of the male specific filamentous phage fd depends upon the presence of a phage protein, specified by fd gene 5, which binds cooperatively to single stranded DNA and denatures double stranded DNA. In spite of these similarities, however, gene 5 protein of phage fd and gene 32 protein of phage T4 probably have different roles in the replication of their respective phages.

Alberts *et al.* knew from the work of others that the fd gene 5 protein is required for the synthesis of progeny genomes but does not form part of the virion, so that they labelled fd infected and uninfected *E. coli* with ^{14}C and ^3H -arginine respectively, made extracts of the cells and passed them through single strand DNA-cellulose affinity columns to pick out any fd protein which binds to single stranded DNA. Sure enough they found such a protein which has a molecular weight of 10,000 and of which there are about 150,000 copies in each fd infected cell. Alberts *et al.* then used amber and temperature sensitive mutants of fd to prove that this protein is indeed specified by gene 5 of fd.

Alberts *et al.* then established, using the techniques developed to characterize T4 gene 32 protein, first, that the binding of fd gene 5 protein to single stranded DNA is highly cooperative and, second, that one protein molecule binds for about every four DNA bases. At this point Alberts *et al.* probably thought that the fd protein was virtually identical to the T4 protein, and the electron micrographs of gene 5 protein bound to fd DNA taken by Delius may have come as something of a surprise. When gene 5 protein binds to single stranded circular fd genomes it causes

them to coalesce in pairs into helical, rod-like structures; it does not force the DNA into extended linear conformation as does T4 gene 32 protein.

From these and other data it seems likely that one of the functions of gene 5 protein is to coat newly synthesized progeny single stranded genomes, which are never found free in infected cells, and thereby prevent them from being immediately converted into more double stranded replicative forms. Presumably during the final encapsidation of progeny fd particles, which occurs at the cell membrane of the infected cells, the gene 5 protein is replaced by authentic coat protein.

Oey and Knippers have independently isolated gene 5 protein and characterized some of its properties. Their estimates of its molecular weight, the number of copies in each infected cell and its binding properties are essentially identical to those reported by Alberts. Furthermore, Oey and Knippers have found that gene 5 protein partially blocks the activity of *E. coli* exonuclease I and completely inhibits exonuclease III, and at low concentrations it promotes the template activity of the fd replicative form DNA for *E. coli* DNA polymerase II.

At this early stage in the characterization of the fd gene 5 protein, the various functions, apart from simply protecting progeny single strands from being converted into replicative forms,

that have been ascribed to it remain. The protein might, for example, act as a repressor for the synthesis of the strand complementary to the progeny viral strand in some rolling circle intermediate—an idea proposed by Marvin's group—or it may be part of the replication apparatus which synthesizes progeny viral strands asymmetrically. No doubt when more is known of the properties of this protein, and of the enzymology of replication of single-stranded DNA, some of these ideas will either be eliminated or substantiated.

STELLAR RADIATION

Infrared Excesses

from a Correspondent

SINCE the discovery of stars which emit unexpectedly large amounts of energy in the infrared ($\lambda \gtrsim 1 \mu\text{m}$) it has been thought that the emission arises primarily from thermal re-radiation by circumstellar dust of absorbed stellar radiation. This view came under serious challenge at the symposium on infrared excesses in stars held at Santa Cruz under the auspices of the Astronomical Society of the Pacific on June 26–29.

A stimulating review by Drs H. M. Dyck and R. W. Milkey (Kitt Peak National Observatory) suggested the importance of electron-proton and elec-

Growth of Aquatic Plants

THE promotion of stem extension in plants is generally thought to be regulated by two groups of growth hormones—the auxins and the gibberellins. Two reports to be published shortly in *Nature New Biology* show that the so-called growth inhibitory hormones ethylene and abscisic acid may actually regulate shoot growth in aquatic plants.

In *Nature New Biology* (July 19) Musgrave, Jackson and Ling report their findings on internode elongation in *Callitriche platycarpa*. This is a common freshwater perennial which consists of a floating rosette of leaves and telescoped internodes, attached to the waterbed by a slender stem. If the rosette is submerged then the growth of the immature internodes is greatly enhanced. McComb previously attributed this pattern of growth to enhanced synthesis of, or sensitivity to, gibberellin (GA) in the submerged rosettes. Musgrave *et al.* report that the sensitivity of the tissue to gibberellin is increased by ethylene and the quantity of ethylene to which the plant is subjected rises when the rosettes are submerged. This may relate to slower diffusion of the ethylene out of the air spaces in the leaves when the plant is totally immersed in water.

Musgrave *et al.* find that GA_3 promotes internode elongation but not to the same extent as submergence, whereas ethylene treatment is at least as effective as submergence. Applied ethylene gas stimulates internode elongation of floating rosettes but has no effect when they are submerged. On the other hand, ethylene applied with GA_3 produces a greater response than submergence alone. Ethylene could, therefore, increase the response to GA by enabling the plant to achieve its full potential for elongation.

Musgrave *et al.* also briefly mention the stimulation of petiole growth by ethylene in the aquatic form of the celery-leaved buttercup, *Ranunculus sceleratus*. This finding, taken together with the results of earlier workers on the semi-aquatic plant, rice, would indicate that the regulation of cell extension by ethylene may be a common feature of aquatic plants. In the same issue of *Nature New Biology* Takahashi's report on mesocotyl growth in rice seedlings suggests that abscisic acid may also act as a stimulator of extension growth. It is significant that in both articles the action of ethylene and abscisic acid is seen in terms of an effect of gibberellin metabolism.

tron-neutral hydrogen bremsstrahlung (free-free emission) in envelopes surrounding stars of early type. They presented convincing fits to the infrared spectral energy distributions of a wide variety of early-type stars by assuming the superposition of a low temperature free-free emitting gas shell on an underlying stellar spectral energy distribution. Dyck and Milkey proceeded from purely theoretical considerations to estimate circumstellar envelope parameters. Typical values of temperature, neutral hydrogen density and sizes were derived to be 3,000 K, 10^{12} atoms/cm³ and 100–1,000 times a typical stellar radius respectively.

The importance of gas emission in the interpretation of infrared excesses received further support from the report of a strong correlation between hydrogen line emission, Balmer continuum emission and observed flux in the infrared. This correlation was found by Drs S. E. Strom (Kitt Peak National Observatory) and L. V. Kuhl (Berkeley Astronomy Department) to apply to a wide variety of stars in the temperature range 4,000 to 20,000 K.

There seems no doubt, however, that dust plays an important, although certainly not exclusive, part in the infrared emission properties of many objects at all spectral types. Dr E. P. Ney (University of Minnesota) presented an impressive array of high resolution observations of the so-called 11 μ m "bump" attributed by many authors to emission by silicate dust grains. Although this feature has been observed for some time in the infrared spectra of many oxygen-rich stars of late spectral type, Dr Ney also presented evidence that the silicate feature occurs in a few of the early-type, hot stars in Orion I, the very young stellar association. The dust envelopes producing this feature appear to be cool (a few hundred degrees K) and quite extended (angular size 20 arc seconds corresponding to a linear size of about 10^{17} cm). It seems most likely that the dust surrounding these early-type stars is remnant material from the cloud out of which the star was formed.

Dr N. J. Woolf (University of Minnesota) also reported the observation of the 11 μ m silicate feature in absorption against the Becklin-Neugebauer source in Orion and against the infrared core of the galactic centre. An absorption feature at 3.1 μ m was also seen against the Orion source; if this feature is attributed to interstellar ice, the ice-to-silicate ratio is estimated to be between 1/6 and 1/20. Dr Woolf's observations suggest that silicate material is the principal constituent of the interstellar dust grains. Whether all the silicates found in the interstellar medium have been created in the envelopes of late-type stars still seemed to be open to question. The

identification of the 11 μ m feature as a silicate signature received considerable support from a detailed laboratory analysis of the optical properties of almandine garnet by Dr D. R. Huffman and his associates (University of Arizona).

Dr J. E. Gaustad (Berkeley Astronomy Department) presented new, high resolution observations made with a Fourier transform spectrometer. His data revealed an emission feature at 12 μ m in several carbon-rich late-type stars which he attributed to SiC. This identification is made plausible not only by the wavelength match but by the expected abundance of SiC as a condensate in the envelopes of carbon-rich stars.

Dr Strom reviewed the observed infrared properties of extremely young stellar objects. Infrared excesses are observed in young stars of all spectral types; these excesses seem to result from

a combination of free-free and dust radiation possibly from remnant material in circumstellar envelopes. Some combined infrared and optical spectroscopic evidence suggests that these envelopes are not spherical, but possibly disk-like; this view finds some support from the polarimetric data presented by Drs B. Zellner and K. Serkowski (University of Arizona). Many of the details of pre-main sequence stellar and envelope evolution, however, are not yet clear. The appearance of mass outflow in most young stars of all types runs counter to the hydrodynamical models of pre-main sequence evolution proposed by Dr R. Larson (Yale University) in which a stellar core gradually accretes material from the surrounding envelope. Considerably more effort could well be applied to the identification and spectroscopic study of stars younger than those found in young

Cholesterol and Catecholamine Uptake

A POSSIBLE rationale for the fashionable condemnation of high intakes of the steroid cholesterol as a causative factor in hypertension is hinted at by P. J. Salt and L. L. Iversen, from the University of Cambridge, in a report in next Wednesday's *Nature New Biology* (July 19). They show that cholesterol inhibits the extraneuronal uptake of catecholamines.

Catecholamine uptake, rigorously explored in the 1960s, occurs by two processes. In sympathetically-innervated organs, perfusion with exogenous radioactively-labelled catecholamines results in rapid labelling of the tissue. Iversen, using the isolated rat heart, perfused through the coronary vasculature, has found that with low concentrations of noradrenaline, most of the incorporated label entered postganglionic sympathetic nerve terminals; in hearts taken from animals lacking such terminals as a result of immunosympathectomy at birth, uptake was almost completely absent. But perfusion of rat hearts with substantially higher concentrations of catecholamines showed that, in addition to uptake by neuronal sites (termed uptake 1), transfer to cardiac muscle cells also took place, by a process (uptake 2) with properties clearly distinct from those of uptake 1. Thus, uptake 2, unlike the neuronal process, has a higher affinity for adrenaline than noradrenaline. Similarly, catecholamines entering extraneuronal sites are not firmly retained, whereas the uptake into terminals is followed by transfer to intraneuronal storage vesicles, and therefore cannot readily be reversed.

Physiologically, both uptake processes are important for preventing released catecholamines from continuing to act after their effect is complete. Noradrenaline is the transmitter released at

adrenergic nerve terminals, and thus its recapture by the uptake 1 process ensures its removal from extracellular sites as well as permitting the replenishment of storage vesicles. Uptake 2 followed by metabolism may also play some part in the inactivation of transmitter noradrenaline, but is much more likely to be predominantly involved in the removal and inactivation of circulating catecholamines by way of the smooth muscle of blood vessels.

The two uptake processes also differ in their susceptibility to drugs. Uptake 1 is inhibited by a range of sympathomimetic amines related to noradrenaline, many of which have little effect on uptake 2. Conversely, Iversen and Salt, in a previous article (*Brit. J. Pharmacol.*, 40, 528, 1970), have shown that certain steroids are highly effective and specific inhibitors of uptake 2. These inhibitors include corticosterone, 17- β -oestradiol and deoxycorticosterone, all of which occur naturally but at plasma concentrations below those required to cause an effective inhibition of uptake under normal physiological conditions. Cholesterol is present in plasma at a much higher concentration, however, and, although as described by Iversen and Salt in their present article, it is a less potent antagonist of uptake 2 than any of the steroids previously investigated, it could exert a significant inhibitory influence on the effectiveness with which catecholamines are removed from the circulation under conditions encountered physiologically.

This blockade would be more marked when the plasma cholesterol is increased, for instance as a result of a high dietary intake, and in this way cardiovascular changes leading to the development of hypertension could be brought about.

clusters. Moreover, theoretical efforts to understand mass outflows in young stellar objects outside the T-Tauri regime would seem to be essential.

The observational results summarized at the symposium demonstrated quite dramatically the growing quantity and quality of infrared data for a wide variety of stellar objects. As summarized by Dr W. Stein (University of California, San Diego), these data now demand application of far more sophisticated theoretical tools for proper interpretation. Much more detailed treatments of radiative transfer in circumstellar gas and dust envelopes of various geometries are particularly needed. Detailed laboratory analysis of the optical properties of many of the more abundant solid constituents of circumstellar dust envelopes would also be valuable.

PALAEONTOLOGY

Seminar on Carbonates

from a Correspondent

A NEW experiment in "in-service teaching" was successfully organized by Dr J. A. E. B. Hubbard (King's College, London) for the Palaeontological Association at the Robertson Research Co. Ltd Laboratories, North Wales, from June 23–26. The seminar, which was about processes in carbonate environments, attracted thirty palaeontologists and was led by Dr R. G. C. Bathurst (University of Liverpool).

The Palaeontological Association had felt that there was a need for palaeontologists to have refresher courses in associated parts of geology. The field of carbonates was an excellent choice for a first seminar, for there have been many developments in the subject in the past five to ten years, particularly in regard to the processes and effects of diagenesis, the study of reefs and the relationships between carbonate and associated evaporite rocks. Many of these problems have been approached through studies of recent sediments.

The first day of the seminar was devoted to submarine sediments but dealt chiefly with coral reefs. Dr T. P. Scoffin (University of Edinburgh) showed how the study of present day Bermuda patch reefs had helped his interpretation of ancient reefs from Wenlock Edge in Shropshire. Dr J. Schröder (Technical University of West Berlin) developed a general model for reefs based on his work on Bermuda cup reefs. Both speakers stressed the important balance between the growth of the corals and algae which build the reef and the boring shells, rasping fish and the like, which are eroding it simultaneously. Dr Hubbard showed the first of several excellent underwater cine films taken in Florida. These submarine traverses help in understanding sediment

distribution and its control and should be obligatory viewing in all undergraduate carbonate sediment courses.

On the second day participants looked at intertidal and supratidal sediments. Dr P. Garrett (University of Leeds) has been studying recent intertidal carbonates in the Bahamas and showed how their structures and binding by algal mats are governed by the length of time they are exposed by seawater. Moving to a more arid part of the world, Dr P. Bush (Imperial College, London) described the carbonates and associated anhydrite and gypsum evaporites which are now forming in the Abu Dhabi area of the Arabian Gulf. The similarities between these sabkha deposits and many ancient rocks (for example, the Jurassic rocks of the Dorset coast) are most interesting. This type of evaporite deposit is quite thin (measured in tens of metres) and there is still much debate about the origin of the thick evaporites (hundreds of metres) such as the Permian ones under the North Sea.

Dr J. A. Dickson (University of Nottingham) gave an elegant demonstration of the use of staining methods to reveal hidden variations in limestone fabrics. He later used these methods in an analysis of the seeding and growth of calcite crystals in limestones. He showed that diagenetic studies of limestones should never be based on petrographic studies of single thin sections; several of these from one hand specimen of the Faringdon sponge gravel showed great variability.

On the third and fourth days the discussions turned to the complex processes

of diagenesis which turn the loose carbonate sediment into a limestone rock. During these lectures, and indeed throughout the four days, the importance of the presence of much organic matter in the carbonate sediments was stressed repeatedly. Dr J. D. Hudson (University of Leicester) showed how some fossils on the Jurassic of Skye are so well preserved that original shell microarchitecture is still present. Using this material he pointed to the problems in using carbon and oxygen isotopes to interpret environments of deposition. Dr Schröder illustrated the many cement fabrics that begin to form within the original cavities in a sediment. His scanning electron microscope pictures and microprobe analyses indicate the chemistry of the processes which turn an unstable, organism derived, carbonate mineral assemblage into a limestone rock composed of calcite.

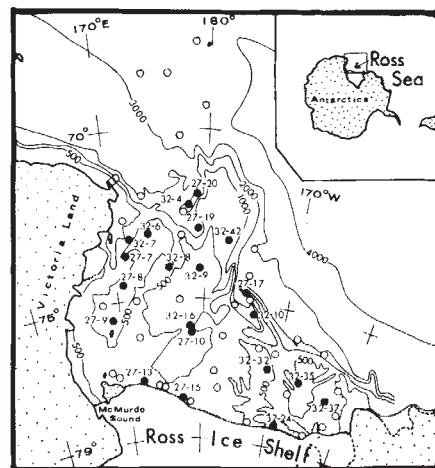
Dr D. J. Shearman (Imperial College, London) showed that the matters discussed were not of purely academic interest. Many petroleum bodies are located in carbonate and evaporite rocks similar to those that had been discussed. His examples ranged from the Ordovician evaporites of Ellesmere Island to the Permian limestones of Texas. Dr Shearman also described the replacement of limestones by gypsum and other minerals. Some of these transformations produce a very porous rock which is ripe for petroleum storage. When capped by a good impermeable evaporite the ideal reservoir rock is produced. Dr Bathurst completed the story of limestone cementation by describing the

Widespread Submarine Erosion in Ross Sea

A WIDESPREAD survey of the Ross Sea bed has been carried out by R. H. Fillon, of the University of Rhode Island, and some results are reported in next Monday's *Nature Physical Science* (July 17). Although the position of this sea, at relatively high latitudes but associated with the Ross Ice Shelf, makes it ideal for studying the relationships between palaeoclimatology and palaeo-oceanography of the Antarctic, only limited surveys have previously been available.

Fillon's study is based on sediments obtained from sixty-four cores taken from within and just north of the Ross Sea (see figure—both open and solid circles denote core locations). He finds evidence for widespread submarine erosion, with most sediment younger than 2 m.y. having been removed, and only little deposition having taken place recently. The cause of this may be a scouring action resulting from increased velocities of the bottom water currents produced by the growth of the Ross Ice Shelf. These and other data point

to a change in climatic conditions in the area occurring abruptly about 2 m.y. BP, corresponding closely with the results of similar studies of the Tasman Sea (see, for example, N. D. Watkins and J. P. Kennett, *Science*, **173**, 813; 1971).



Map of Ross Sea. Bathymetric contours are marked in metres.

effects of freshwaters. It is at this stage that much of a limestone's original porosity (about 50–60 per cent) is filled in with calcite. This process requires a large amount of calcium carbonate.

STERIODS

How ACTH Works

from a Correspondent

A TIMELY and interesting symposium on the mode of action of adrenocorticotrophin (ACTH) was organized by the Steroid Group of the Biochemical Society at the University of Aberdeen on June 2. In the first contribution, Dr M. Golder and his colleagues (Tenovus Institute for Cancer Research, Cardiff) described their combined histochemical and biochemical studies on the adrenal cortex. Dr Golder reviewed the vascular system of this gland and correlated certain aspects of structure and function. The cell migration theory was contrasted with the zonation theory and the morphological effects of ACTH in this gland were shown to be associated with a decrease in the fasciculata cells; this suggests that these cells respond to ACTH.

Dr Golder and his colleagues have applied the ultramicro techniques of Glick and associates to study the distribution of 11β -hydroxylase activity in rat adrenal. By using iodinated ACTH, it was possible to ascertain the distribution of this hormone in various guinea-pig tissues. In this species the uptake of labelled ACTH appeared to be greater in the outer zones of the adrenal cortex. It was shown that ACTH can activate *in vitro* the adenyl cyclase activity of guinea-pig adrenal cortical sections and, by coupling these histochemical methods to precise statistical analyses, Dr Golder demonstrated that ACTH affects the adenyl cyclase system in the fasciculata and the reticularis without influencing the activity of adenyl cyclase in the glomerulosa.

Dr D. Schulster and his colleagues (University of Sussex) reviewed current views on the mode of action of ACTH in the adrenal gland; these ideas evolved largely from the studies of the late Dr L. D. Garren. Dr Schulster went on to describe his studies using isolated adrenal cells: decapsulated rat adrenals are treated with collagenase, filtered through nylon mesh, washed, centrifuged and incubated. By the application of a radioimmuno-assay for corticosterone it has been possible to use this adrenal cell preparation to study the mode of action of ACTH. Dr Schulster then went on to discuss the apparent anomaly that when the cells are stimulated by ACTH the corticosteroidogenesis can be maximal at a concentration of ACTH at which the intracellular cyclic AMP concentration

is still markedly sub-maximal. The time lag of 0–3 minutes before steroidogenesis is switched on is not caused by a lag in cyclic AMP production because the rise in cyclic AMP concentration on exposure to ACTH is almost instantaneous. Dr Schulster considered that his experiments confirmed the idea that this "time lag" may be the consequence of the time required for translation of the messenger RNA for a specific protein.

Dr Schulster showed that there is a good correlation between the dose of cycloheximide required to give 50 per cent inhibition of steroidogenesis with the dose of cycloheximide required to inhibit protein synthesis in these isolated cells. He has found that at certain concentrations of cycloheximide, the time lag for steroidogenesis to commence doubled and the rate of steroidogenesis was halved. Dr Schulster suggested that the labile protein produced as a consequence of the ACTH stimulation may have a half-life of about 4 minutes.

Dr L. R. Johnson (University of Utah) discussed the effect of ACTH on the early sterol and steroidal events in the adrenal cortex—namely, the mitochondrial conversion of cholesterol to pregnenolone. He described the development in his laboratory of a radioimmuno-assay of pregnenolone and discussed the sensitivity and specificity of this radioimmuno-assay. He then reviewed the various phases of the electron transport system in adrenal mitochondria associated with the steroid hydroxylation reactions in the

conversion of cholesterol to pregnenolone. It has been suggested that ACTH might facilitate the efflux of pregnenolone from mitochondria. Dr Johnson has found, however, that there seems to be no inhibition of secretion of pregnenolone by adrenal cortical mitochondria and that it seems unlikely that ACTH is involved in the mitochondrial pregnenolone efflux phase of steroidogenesis.

Dr E. R. Simpson (University of Edinburgh) reviewed the electron transport system in adrenal cortical mitochondria and discussed the lipid content of adrenals with special reference to the cholesterol ester content of these glands. He reviewed the evidence which implicates cholesterol esters as the precursors of the free cholesterol required in steroidogenesis. He has confirmed that cycloheximide administered to rats *in vivo* fails to block the ACTH-induced cholesterol esterase activity of the adrenal glands. He showed that in bovine adrenal cortex tissue the cholesterol esterase in the cell supernatant in the presence of a protein kinase and cyclic AMP together with ATP gave a three-fold increase in the specific cholesterol esterase activity. Dr Simpson then discussed the effect of ether anaesthesia as a means of increasing the plasma ACTH concentration in rats. Under ether anaesthesia, the protein kinase activity of rat adrenal cortical tissue was significantly increased. In rat adrenal mitochondria there is a biphasic response in pregnenolone formation and Dr Simpson went on to

Genetic Heterogeneity of Xeroderma Pigmentosum

XERODERMA pigmentosum is one of those fortunately rare genetic defect diseases which have captured the interest of human geneticists and provided them with useful experimental material. The chief symptom of the syndrome is an extreme hypersensitivity of the skin to ultraviolet radiation resulting apparently from an autosomal recessive mutation that impairs a cell's DNA repair machinery. Patients with the classic symptoms develop severe skin lesions, but a few suffer from the De Sanctis-Cacchione syndrome which includes neurological and mental deficiency. This heterogeneity of symptoms suggests, of course, that the genetic defects resulting in xeroderma pigmentosum might involve more than one gene; that this is almost certainly the case has now been established by Bootsma's group in Rotterdam.

Writing in next Wednesday's *Nature New Biology* (July 19), Weerd-Kastelein, Keijzer and Bootsma report measurements of DNA repair capacity of heterokaryons produced by fusing in pairs cultivated fibroblasts from men

and women suffering from either classic xeroderma pigmentosum or the De Sanctis-Cacchione syndrome. Their data indicate that mixed heterokaryons obtained by fusing xeroderma pigmentosum cells with De Sanctis-Cacchione cells marked by sex chromosomes are better able to repair DNA damaged by radiation than are heterokaryons obtained by fusing pairs of xeroderma or De Sanctis cells. Furthermore, both the nuclei in the mixed heterokaryons incorporate ^3H -thymidine after irradiation.

The simplest and obvious interpretation of these findings is that two different genes each specifying different proteins are required for repair of radiation damaged DNA, and that in classic xeroderma pigmentosum one gene is inactivated and in the De Sanctis-Cacchione syndrome the other gene is inactivated. In the mixed heterokaryons, intergenic complementation would restore the repair function. By isolating and characterizing the properties of the repair enzyme it should be possible to confirm this.

show that ether anaesthesia affected this response.

Dr Simpson suggested that ACTH may affect the amount of substrate-cholesterol associated with the key cytochrome P_{450} which is involved in the cholesterol side-chain cleavage reaction. Various spectral changes in cytochrome P_{450} were interpreted as the conversion of a cholesterol complex of cytochrome P_{450} in a high spin form being converted to a low spin form of this haemoprotein as the cholesterol is converted to pregnenolone. From these studies Dr Simpson put forward the view that an intramitochondrial transport of cholesterol may be a rate-limiting event in the cholesterol side-chain cleavage reaction in adrenal mitochondria.

LEPIDOPTERA

Not So Ephemeral

from a Correspondent

To the romantic at least it is somehow all too appropriate that animals as exquisitely and delicately beautiful as some adult moths and butterflies should be ephemeral, disdaining to ingest anything but nectar and living only long enough to reproduce their kind. And to the many people for whom the ephemerality of these creatures is part of their beauty, the news that at least some species ingest amino-acids as well as sugars and survive for several months may be positively unwelcome. But that undeniably is the case; as Banziger has reported, at least one Malayan species of moth shares with the mosquito a taste for a blood meal, and now Gilbert (*Proc. US Nat. Acad. Sci.*, **69**, 1403; 1972) has found that some species of the attractive neotropical genus of butterflies *Heliconius* (Nymphalinae) ingest and assimilate amino-acids extracted from collected pollen, which they can neither chew nor ingest. For these *Heliconius* species it seems that pollen rather than nitrogenous fat body laid down by the gorging caterpillar is the chief source of amino-acids for egg protein.

Observations of the behaviour of *H. erato* and other related species in the field, the insectary and the greenhouse provided the first suggestive evidence that these butterflies actively collect pollen for food rather than by accident as a result of drinking nectar. For one thing these *Heliconius* species visit flowers for much longer times than other species and repeatedly scrape the tips of their proboscis over anthers, accumulating loads of pollen visible to the naked eye. For another thing several *Heliconius* species exhibit a curious behaviour by which they appear to mix the dry mass of pollen with fluid, presumably nectar, exuded from

the tip of the proboscis, using the proboscis as a whisk. Moreover, the proboscis seems adapted to pollen feeding, for it has papillae at the tip, which are probably chemo-mechanoreceptors, and large mechanoreceptor hairs at the head. Certainly any suggestion that the pollen is collected accidentally is ruled out by experiments Gilbert made with artificial flowers containing minute glass beads instead of pollen; *Heliconius* adults collect these beads as if they were pollen.

How do the butterflies assimilate amino-acids from pollen without chewing or ingesting it? They seem to achieve this by exploiting the fact that when pollen is mixed with sucrose solution most of the free amino-acids in the pollen are rapidly leached into solution. By blending the pollen with nectar the *Heliconius* butterflies provide themselves with a sugary solution of amino-acids, and experiments with solutions of labelled amino-acids fed to these butterflies prove that free amino-acids ingested in solution are rapidly and efficiently assimilated into egg proteins. Furthermore, counts of the numbers of eggs produced by female butterflies reveal that during a period of five days females fed pollen and nectar lay about five times more eggs than females fed nectar alone.

These data clearly indicate that adult butterflies of *Heliconius erato* and probably other species rely on pollen for amino-acids for their eggs and the biology of these animals is consonant with that claim. They have, for example, life spans in the wild and in insectaries of 6 months during which period the females lay eggs at a low



Heliconius ethilla bearing large pollen load collected in less than 3 minutes from a single male flower of *Guarania spinulosa* near Arima Pass, Trinidad (from *Proc. US Nat. Acad. Sci.*, **69**, 1404; 1972).

(1–10 per day) but steady rate and the males remain sexually active. (Gilbert suggests that proline present in high concentration in pollen may act as an energy store and source.)

Gilbert draws attention to the evolutionary advantage of dispersing offspring in time and of the coevolutionary advantages of pollen feeding not only to the *Heliconius* butterflies but also to the plants from which they are able to collect limited but continuous amounts of pollen. Whereas some other species of butterfly may supplement their store of nitrogenous food by feeding on dung, urine, rotting fruit and so on, none of these sources is rich enough to be more than a supplement. But pollen, as the *Heliconius* butterflies have discovered, can supply enough amino-acids to support six months of reproductive life.

Mitochondrial RNA Polymerase and Phage T3 DNA

THE transcriptional and translational apparatus of mitochondria from eukaryotic plants and animals is thought to resemble closely its counterpart in bacteria and to be quite distinct from the machinery of gene expression in the nucleus and cytoplasm of eukaryotic cells. Admittedly mitochondrial RNA polymerase (molecular weight 64,000) is a much smaller enzyme than *Escherichia coli* RNA polymerase (molecular weight 450,000), but both enzymes are sensitive to rifampicin and experiments reported in next Wednesday's *Nature New Biology* (July 19) by Richter, Herrlich and Schweiger prove conclusively that the RNA polymerase from the yeast *Saccharomyces cerevisiae* can recognize at least some transcriptional signals which are recognized by the *E. coli* enzyme.

Richter and his associates have separated from isolated yeast mitochondria a cell free system including RNA polymerase which supports coupled tran-

scription and translation. When this system is programmed with DNA of phage T3 two phage specific enzymes are synthesized—lysozyme and an S-adenosylmethionine cleaving enzyme (SAMase). The synthesis of these two enzymes is inhibited by fusidic acid and actinomycin D, but it is inhibited neither by cycloheximide nor by the antibiotic thiostrepton, which blocks bacterial protein synthesis. When phage T7 DNA is used as template, SAMase is not made and this phage does not carry an SAMase gene.

A variety of controls rule out any suggestion that the results of these experiments are artefactual and stem from bacterial contamination of the mitochondria from which the cell free systems were isolated. The SAMase gene of phage T3 is transcribed by the bacterial RNA polymerase when *E. coli* cells are infected, and so whatever transcriptional signals this bacterial enzyme recognizes are also recognized by the mitochondrial enzyme.

Interstellar Organic Chemistry

CARL SAGAN

Laboratory for Planetary Studies, Cornell University, Ithaca, New York 14850

Complex organic molecules that are stable against radiation may pervade interstellar space, with their degradation products a source of those molecules observed. The connexion between biological and interstellar organic chemistry is, however, analogical rather than substantive, and the prospect for interstellar biology is dim. This article is based on Professor Sagan's contribution to a symposium at the National Radio Astronomy Observatory, Charlottesville, Virginia, in October 1971.

THE existence of an interstellar organic chemistry^{1,2} was suggested by the discovery of the microwave lines of formaldehyde and of hydrogen cyanide in the interstellar medium^{3,4}. With the more recent discoveries of cyanoacetylene (an important condensing agent in pre-biological organic chemistry), formamide, methanol, formic acid (perhaps), acetaldehyde and acetonitrile⁵⁻⁹ its existence has become secure. Aldehydes and nitriles are the precursors of amino-acids in the Strecker synthesis and HCN is the precursor of the purine adenine in experiments in pre-biological organic chemistry¹⁰⁻¹².

Most of the interstellar medium is hydrogen and helium, with C, N, O, and Ne present in a total relative abundance by number $\sim 10^{-3}$ and Mg, Si, S, and Fe $\sim 10^{-4}$ to 10^{-5} . Molecular abundances should reflect these atomic abundances. Between clouds of interstellar material (the most common of which have densities $\sim 10^3$ cm⁻³) the density is ~ 0.1 cm⁻³. Recent work on the repopulation by collision of excited states of CH₃OH, CS and H₂O, which have short natural lifetimes, implies densities in some clouds of 10^6 to 10^8 cm⁻³ or larger¹³⁻¹⁵. In contrast to optically large clouds¹⁶, which have densities ~ 20 H atoms cm⁻³, masses of 20,000 solar masses, and radii ~ 20 pc, the much denser clouds determined by radio techniques contain hydrogen molecules. As the mean density of late spectral type main sequence stars is ≥ 1 g cm⁻³, there must be occasional clouds with densities $\geq 10^{15}$ times the highest densities detected directly. Typical kinetic temperatures in an ordinary cloud are ~ 100 K; in a dark cloud, ~ 10 K; and in the regions between clouds $\sim 10^3$ to 10^4 K.

Reliability of Molecular Identifications

Most of the interstellar organic molecules have been found in the large radio source Sagittarius B2 towards the galactic centre, and in such other regions as W51 and the infrared source in the Orion nebula. The number of lines now being discovered is so great that their unambiguous identification will soon be more difficult. Identifications have often been made on the basis of a single microwave line. In two intervals in the millimetre wave region between 85 and 90 GHz and between 110 and 120 GHz at least fourteen lines have been identified at the time of writing, that is, ~ 1 line per GHz. Several more remain unidentified. The total widths of such

lines in, for example, Sgr B2, are ~ 15 MHz. Thus there is a probability of at least a few per cent that a single line of a molecule of interest will randomly coincide with a known line. This calculation assumes that the rest frequencies of the lines have been correctly estimated; if not, the probability of a misassignment increases still further. Now that the number of lines is increasing very rapidly, identification on the basis of a single line must be viewed with considerable caution. Several lines, or hyperfine splitting and isotopic lines, are required for a secure identification. An interesting possibility is the confirmation of many of these molecular identifications by means of orbiting ultraviolet spectrometers which would examine stars at the peripheries of dense molecular clouds.

Organic Synthesis at High Densities

According to Townes¹⁷ a typical dark cloud with ~ 10 H₂ molecules cm⁻³ might have abundances of CO $\sim 10^{-1}$ to 10^{-2} cm⁻³; HCN $\sim 10^{-4}$ cm⁻³; NH₃ $\sim 10^{-3}$ cm⁻³; HCHO $\sim 10^{-4}$ to 10^{-5} cm⁻³; and water in as yet undetermined abundances. This immediately suggests that much of the O, N, and C atoms in the clouds exists in molecules and in grains. In such a cloud 1 pc across there is $\sim 1 M_{\odot}$ of these simple molecules. Assuming a number density ratio of grains to atoms $\sim 10^{-12}$, there is also $\sim 1 M_{\odot}$ of grains in such a cloud.

The interstellar molecules known to exist in late 1971 are strikingly similar to those detected by optical spectroscopy in cometary comas and tails, and to those identified in experiments in pre-biological organic chemistry, starting from such simple precursors as methane, ammonia, and water (see Tables 1 and 2). The radicals detected in cometary spectra look very much like the dissociation fragments of the identified interstellar molecules, and the gas phase products synthesized in experiments in pre-biological organic chemistry bear a very close relation to the interstellar molecules themselves. For example, apart from CO and hydrocarbons, the most abundant gas phase products found in sparkings of simulated primitive atmospheres¹⁸ and in quenched thermodynamic equilibrium computer simulations of such experiments¹⁹ were HCN, CH₃CN, HCHO, and CH₃CHO. All these molecules have now been reported in the interstellar medium³⁻⁹, and all are produced by a variety of energy sources.

It is, however, very difficult to understand the synthesis of such molecules by two-body reactions in the extraordinarily diffuse interstellar medium. Three-body reactions seem to be required, at least to make the more complex molecules, and typical reaction rate constants are known²⁰ to be $\sim 10^{-32}$ cm⁶ s⁻¹. If the reacting constituents were present at densities

Table 1 Comparison of Interstellar and Cometary Compounds

Microwave identifications of interstellar molecules	Optical identifications of cometary radicals
OH, H ₂ O	O, OH, OH ⁺
CO	CO, CO ⁺ , CO ₂ ⁺
NH ₃	NH ₂ , NH, N ₃ ⁺
CN, HCN	CN
HCHO, CH ₃ CN, HC ₂ CN, HC ₂ CH ₃ , many other organics	CH, CH ⁺ , C ₂ , C ₃

of 1 cm^{-3} there would be ~ 1 three-body reaction km^{-3} in the lifetime of the Galaxy. On the other hand, at a reactant density of 10^{11} cm^{-3} , there will be ~ 1 three-body reaction per molecule in 300 yr, a respectable rate; but regions with this density of minor constituents are atypical and are probably collapsing clouds well on the way to star formation if they have sufficient total mass. Also, regions of such high density are opaque to ultraviolet, whereas in regions of low density the photodissociation time scale, calculated below, is much less than the three-body reaction time scale.

Synthesis in Condensing Clouds

A cloud of such composition, collapsing at low temperature, will produce complex molecular species, particularly as the stars which form out of the cloud reach the high luminosity or Hayashi phase of their pre-main sequence stellar evolution. Shock synthesis of organic compounds is known to be of remarkably high efficiency²¹ and should occur within a large condensing cloud after a bright O or B star inside the cloud turns on. Although temperatures in the early solar nebula of our own solar system are expected to be chiefly $\leq 300 \text{ K}$, pressures $\gtrsim 10^{-6} \text{ bar}$ are calculated²²⁻²⁴ and the Sun at one point in its evolution had a luminosity $\sim 10^3$ times its present value (see refs. 25, 26, for example). These conditions are sufficiently similar to those in ultraviolet irradiation experiments as to suggest that the same molecules were produced in the early solar nebula (see Table 2). As well as the simple gas phase products of the sort identified to date in the interstellar medium, much more complex molecules, including amino-acids, sugars and nucleotide bases, have been synthesized. And a characteristic intractable brownish polymer forms in large quantities in all such experiments—in the absence as well as in the presence of liquid water²⁷. Collapsed pre-stellar clouds will probably contain such molecules, as well as the comparable Fisher-Tropsch products which resemble carbonaceous chondritic organics²⁸. Organic chemical reactions in such a pre-planetary cloud occur at low temperatures, in contrast to those in the extended envelopes of red giants where the temperatures are sufficiently high to pyrolyse most organic compounds of interest. As the local star goes through its Hayashi phase, most of the surrounding material must be ejected—probably by electromagnetic and corpuscular radiation pressure—back into interstellar space. If the particle size distribution of interstellar grains turns out to be relatively sharply peaked near radius $a \sim 10^{-5} \text{ cm}$, this might provide additional evidence for the origin of the grains in stellar ejection events: the ratio of radiation pressure to gravity is also peaked when $2\pi a \sim \lambda_{\text{max}}$, where $a\pi\lambda_{\text{max}}$ is the Wien peak of the stellar radiation field. The fraction stable against radiation—for example, grains of intractable polymer—should preferentially survive the outward journey.

Herbig²⁹ has inquired whether such dust ejection during the pre-main sequence evolution of stars of middle to late spectral type might be the principal source of the general population of interstellar grains. The mass of dust in the Galaxy is $\sim 10^{-2}$ the mass of gas, which is, in turn, $\sim 10^{-1}$ the mass of the Galaxy. Thus if $\sim 10^{-3}$ of the galactic mass is dust, $\sim 10^{-3}$ of the mass of each star must be ejected in the form of grains according to this hypothesis. Models of the early solar nebula place its mass between $10^{-2} M_{\odot}$ and $1 M_{\odot}$ (ref. 30), essentially all of which must have been dissipated into space. Thus between $10^{-3} M_{\odot}$ and $10^{-5} M_{\odot}$ of C, N, and O must have been ejected by each star. If the grains are primarily compounds of these atoms, this is barely enough to explain the entire mass of interstellar grains, if we assume that the grain half-life is comparable with the age of the Galaxy and that there is not a significant mass of grains with $a \gg$ or $\ll$ visible wavelengths. Hence, a significant fraction of interstellar grain material may consist of stable organic molecules ejected during star formation. This

Table 2 Simple Organic Compounds of H, C, N and O in the Interstellar Medium and in Experiments on Pre-biological Organic Chemistry

	Interstellar microwave identification	Laboratory identification
CO	✓	✓
C ₂ H ₂	No accessible lines	✓
C ₃ H ₄	No accessible lines	✓
C ₂ H ₆	No accessible lines	✓
HCHO	✓	✓
CH ₃ CHO	✓	✓
HCN, HNC	✓	✓
CH ₃ CN	✓	✓
HNCO, HCNO	✓	✓
CN ₃ OH	✓	✓
C ₂ H ₅ OH		✓
HC ₂ CN	✓	✓
HC ₂ CH ₃	✓	
HC ₂ NH ₂		
HC ₂ CHO		
NH ₂ CHO	✓	✓
CH ₃ NH ₂		
HCOOH	✓	✓
CH ₃ COOH		✓
.		
Higher nitriles and polynitriles		✓
Sugars		✓
Amino-acids		✓
Nucleotide bases		✓
Porphyrins		✓
Polycyclic aromatics		✓
"Intractable polymers"		✓

From refs. 3-12, and B. N. Khare and C. S., to be published.

hypothesis should be distinguished from hypotheses of grain origin within stellar atmospheres—of red giants, for example.

Degradational Origin

Organic material produced in the early history of solar systems must be making contributions to interstellar organic chemistry because of the dissipation of organic molecules during star formation and the subsequent loss of comets to the interstellar medium. Objects of cometary mass, and even grains $\sim 10^{-5} \text{ cm}$, will dissipate in the interstellar radiation environment quite slowly. I therefore suggest that the simple interstellar organic molecules could be released from organic grains by grain-grain and grain-molecule collisions, shock waves, and heat from nearby stars. Single photon events are unlikely to be very effective in grain degradation; from calculations below, it follows that a typical grain will absorb $\sim 10^3$ molecules of ices in the time required for absorption of a single ultraviolet photon. Microwave line astronomers may be merely skimming the surface of the problem, detecting molecular fragments spalled off large organic molecules which pervade the interstellar medium. This view is the antithesis of the more usual one in which the interstellar molecules are imagined to be synthesized within the interstellar medium from yet simpler precursors.

Synthesis on Grains

An alternative possibility is that moderately large interstellar molecules are synthesized on grain surfaces³¹. Because of grain surface defects, there are unbound valence electrons and high sticking probabilities, so free radicals and simple non-fully-saturated organic molecules should grow there. The bond energies connecting these radicals with the grain are comparable with the energies of typical long wavelength ultraviolet photons in the interstellar radiation field. Collisions on the surface and in grain interstices provide a local environment for three-body reactions. The combination of adjacent radicals attached to the grain also occurs by quantum mechanical barrier tunnelling³¹—relatively efficient for H and relatively inefficient for larger radicals. Synthesis on or near grains may not be very efficient for larger organic molecules.

A typical interstellar atom has a 50% probability of having passed through one pre-stellar nebula during the lifetime of the Galaxy; on the other hand, an atom has had $\sim 10^3$ grain collisions during the same period. It is entirely possible that the production efficiency in a nebular environment is $>10^3$ times that on the interstellar grains. (It is even possible that liquid water was available in the solar nebula.) Whether grain or pre-stellar cloud chemistry dominates remains highly uncertain; it seems clear that both will make important contributions to interstellar organic chemistry.

Khare and Sagan (to be published) have recently ultraviolet irradiated condensates of simple interstellar molecules (H_2O , NH_3 , HCHO , C_2H_6 frozen at 77 K on a quartz finger. The results are not expected to depend sensitively on the energy source employed. Some products are synthesized at 77 K demonstrating low temperature radical mobility in such systems. When the irradiation products were heated (as, for example, in the dissipation of a grain or of a prestellar cloud), a range of more complex organic molecules were detected—some of which (like HCN , CH_3CN , CH_3CHO , and CH_3OH) have been identified in the interstellar medium.

Ultraviolet Natural Selection

In free interstellar space the radiation field is dominated at short wavelengths by bright O and B stars, and has an energy density $\sim 10^{-13}$ erg cm^{-3} in the photolytic ultraviolet, peaked at a little less than 2000 Å. With a typical photon energy ~ 10 eV this energy density is $\sim 10^{-2}$ photons cm^{-3} , and corresponds to a flux $F \sim 3 \times 10^8$ photons $\text{cm}^{-2} \text{s}^{-1}$. The mean lifetime, t_r , of the molecule is $\sim (\sigma F)^{-1}$, where σ is the mean photodissociation cross section over the wavelength region in which the mean flux is specified. For most of the molecules in question, $\sigma \sim 10^{-18}$ cm^2 (see, for example, ref. 32), and $t_r \sim 100$ yr. Some of the identified molecules, such as HCHO and CH_3CHO , and some plausible candidates, such as CH_3COCH_3 , H_2S , $(\text{CH}_3\text{S})_2$ and $(\text{C}_2\text{H}_5\text{S})_2$, have non-trivial absorption features near 3000 Å; because more photons are available to photodissociate such molecules, their lifetimes should be correspondingly shorter. Carbon monoxide, on the other hand, is not dissociated until 1110 Å (ref. 32) where σ is relatively low. Accordingly t_r for CO is about ten times larger than for aldehydes. Solomon¹⁴ finds, for Heiles Cloud II, a sharp decline in the HCHO abundance at the cloud boundary, whereas CO can be found at considerable distance outside the cloud. This is in accordance with our rough calculations and emphasizes the importance of the interstellar radiation field in determining which molecules will be present. Similarly, despite the comparable cosmic abundances of nitrogen and oxygen the radio line ratio $[\text{CN}]/[\text{CO}] \sim 10^{-3}$ to 10^{-4} ; this is consistent with the bond energies and values of σ for the cyanyl radical compared with CO.

Within clouds the ultraviolet flux may be substantially attenuated, and the lifetime of the molecules correspondingly increased. For an extinction of A_λ mag, the attenuation will be $10^{-0.4A_\lambda}$. Lifetimes of typical dense clouds are $\sim 10^7$ yr, so for t_r of the contained molecules to be approximately the lifetime against dissipation of the cloud in which they reside, the ultraviolet flux must be down by a factor $\sim 10^4$, corresponding to ~ 10 mag extinction in the ultraviolet, which is not excessive. Accordingly, the association of complex interstellar molecules with clouds is to be expected: they are produced there, and they are preserved there. But because the clouds themselves have lifetimes short compared with the age of the Galaxy, the contained molecules must either have been made in a period $\leq$ the lifetime of the cloud, or have been transported to the cloud from elsewhere by means which protect the molecules from ultraviolet radiation. Star formation, and ejection of organic grains stable against radiation, provide a natural solution to this problem.

Some radio astronomers suspect that ultraviolet-labile molecules are being seen at greater distances from dense clouds than would be expected on the basis of their lifetime t_r . It is difficult to be certain about such a matter because nothing is known about possible very small dense clouds; but if grains are composed of organic compounds, their ultraviolet degradation readily provides small organic molecules far from dense clouds.

These examples of molecular differential stability suggest the occurrence of a kind of ultraviolet natural selection in the interstellar medium. Those molecules which are relatively stable to interstellar ultraviolet radiation may eventually achieve an abundance much higher than their relatively small production rates might suggest. As $t_r \lesssim 10^8$ yr for small molecules, a molecule that is very stable against radiation can achieve significant abundance even if its production rate is down by a factor of 10^6 or 10^7 . It is therefore most important to discover which molecules have such properties.

There have been no systematic studies of the prolonged irradiation of cosmic gases, but preliminary results (ref. 26, for example) indicate that there is a significant trend towards high molecular weight material—the intractable polymers, produced in relative yields $\geq 10^{-8}$. It is not difficult to understand such results³³. First, large molecules are, all other factors being equal, more stable than small molecules: if a single bond is broken in a diatomic molecule, its constituent atoms fly apart and restructuring the molecule is unlikely, but if the same bond is broken in a polyatomic molecule the steric forces provided by adjacent atoms will tend to heal the break. Second, the π electrons in heterocyclic organic molecules are able to absorb in the ultraviolet, as an excitation rather than as a dissociation event, much more readily than simpler molecules can. For these reasons cyclic compounds of high molecular weight may be an important constituent of the interstellar grains and gas. Such molecules are found in the carbonaceous chondrites, which are probably the best samples of the Sun's pre-stellar nebula available to us, and which are possibly³⁴ the most abundant variety of meteoroids in interplanetary space.

Interstellar Biology

The existence of moderately complex organic molecules in the interstellar medium suggests that such molecules may in some way be connected with biological processes somewhere. There are two principal possibilities: first, that such molecules support or are metabolic products of an interstellar biota; and second, that such molecules, participating in planetary condensation from the interstellar medium, can make a significant contribution to the origin of planetary life. I shall consider these possibilities in turn.

There is no *a priori* objection to interstellar organisms. With the temperature difference between the 10^4 K diluted interstellar starlight radiation field and the 3 K black-body background radiation, a heat engine with efficiency $>99.9\%$ can be driven; and the existence of interstellar masers and refrigerators shows that large departures from thermodynamic equilibrium, such as characterize living systems, are not uncommon in the interstellar environment. I shall consider a hypothetical interstellar organism with $a \sim 10^5$ cm—about the size of a rabies virus or the smallest known free living organism, PPLO—and suppose that this organism resides in a typical diffuse cloud with a total number density ~ 1 cm^{-3} , and with gas kinetic velocities $v \sim 1$ km s^{-1} . The organism must be mostly of C, N, and O, or atoms of lower abundance, the density of which is assumed to be $n \sim 10^3$ cm^{-3} . The mean free time between collisions of the putative organism with such "heavy" atoms, $t_c \sim (n\sigma_c v)^{-1} \sim 1$ yr, where σ_c is the collision cross section. If the organism is assumed spherical, with a density of ~ 1 g cm^{-3} , it contains $N \sim 2 \times 10^8$ heavy atoms. Assuming an accommodation coefficient of unity, so that all incident atoms stick, these calculations imply a

replication time scale $\tau \sim 2 \times 10^8$ yr. Thus there could only have been about fifty generations in the history of the Galaxy, far too little time to evolve, through natural selection, an organism able to function in so inclement an environment. It is difficult to imagine the organism smaller than we have assumed; a larger organism only compounds the difficulty — σ_c increases as a^3 but N increases as a^2 . It is unlikely that v is in error by as much as a factor 10. The only flexibility is in n ; if n is 10^5 times larger (implying a moderately dense cloud), then τ is reduced to $\sim 2 \times 10^8$ yr. But the cloud lifetime is only $\sim 10^6$ yr, yielding about 500 generations, which is still not enough. In fact, the number of generations increases as $\sim n^{1/2}$. There is also, however, a residual possibility that organisms might arise in very dense clouds, or on planets or satellites of low mass where the evaporation of the atmosphere permits gradual adaptation of indigenous organisms to interstellar conditions; they might then be launched back into the interstellar medium by mechanisms similar to those described earlier for grains.

Contributions to Planetary Biology

Can interstellar organic molecules, entrapped in a condensing cloud, make a significant contribution to the origin of life on planets formed from cloud? If such a contribution is to be significant it must compare with the production of organic compounds in primitive planetary atmospheres. Laboratory measurements of ultraviolet quantum yields and models of early solar evolution suggest²⁷ that ~ 200 kg cm⁻² of organic molecules were synthesized in the first 10^9 yr of the Earth's history. Comparable quantities are expected from hypervelocity shocks²¹. Although much larger quantities of organics are available in condensing clouds, it is unlikely that these will survive planet formation³⁵. The concentration of radioactive elements with large ionic radii near the lunar surface argues for early melting of the Moon³⁶, as does its basaltic composition; and radioactive dating of material from the Fra Mauro Apollo 14 site indicates extensive lunar remelting 3.85×10^9 yr ago³⁷. The energy necessary for the initial melting of the Moon is likely to be the gravitational potential energy of accretion, particularly if the Moon accumulated rapidly so that very large quantities of potential energy were not radiated away. The later melting events are attributable to radioactive decay of elements concentrated by earlier melting events at the lunar surface, and similar events must have occurred on all larger objects. Studies of meteoritic irons indicate that melting events occurred in the parent bodies of these objects, which were almost certainly smaller than the Moon (although the survival of carbonaceous chondrite organics implies that their parent bodies remain cool). The specific gravitational potential energy of accretion is GM/R where G is the Newtonian gravitational constant, M the mass of the object and R its final radius. Assuming that all such objects have approximately the same density, as is true throughout the solar system, $GM/R \propto R^2$. The maximum gravitational potential energy per gram is released in the final stages of accretion, and more is released in larger objects. Thus maximum melting should occur near the end of planetary formation. Melting requires temperatures $\geq 1,200^\circ$ C, but temperatures $1,000^\circ$ C, cooler are still adequate to denature, pyrolyse or thoroughly dissociate all organic molecules of interest. Alternative proposals, for example that the Moon accreted from already differentiated material³⁸, do not enhance the survival of interstellar organic molecules. Interstellar molecules were certainly accreted subsequently—say, in the first 10^9 yr after planetary formation—but the exogenous supply does not approach the indigenous supply from atmospheric chemistry. It also seems implausible that there is a specific molecule, required for the origin of life, which can be synthesized only under interstellar conditions.

Accordingly, the contribution of interstellar organic

chemistry to problems in biology is not substantive but analogical. The interstellar medium reveals the operation of chemical processes which, on Earth, and perhaps on vast numbers of planets throughout the universe, led to the origin of life, but the actual molecules of the interstellar medium are unlikely to play any significant biological role.

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- ¹ Sagan, C., in *Highlights of Astronomy*, 2, 436, 349 *Trans. Intern. Astron. Union* (D. Reidel, Dordrecht, 1971).
- ² Sagan, C., and Khare, B. N., *Bull. Amer. Astron. Soc.*, 2, 340 (1970).
- ³ Snyder, L. E., Buhl, D., Zuckerman, B., and Palmer, P., *Phys. Rev. Lett.*, 22, 679 (1969).
- ⁴ Snyder, L., and Buhl, D., *Astrophys. J.*, 163, L47 (1971).
- ⁵ Turner, B., *Astrophys. J.*, 163, L35 (1971).
- ⁶ Zuckerman, B., Ball, J., and Gottlieb, C., *Astrophys. J.*, 163, L41 (1971).
- ⁷ Ball, J., Gottlieb, C., Lilley, A., and Radford, H., *Astrophys. J.*, 162, L203 (1971).
- ⁸ Solomon, P., Jefferts, K., Penzias, A., and Wilson, R., *Astrophys. J. Lett.* (in the press).
- ⁹ Ball, J., Gottlieb, C., Lilley, A., and Radford, H., *IAU Circular*, No. 2350 (1971).
- ¹⁰ Calvin, M., *Chemical Evolution* (Oxford University Press, Oxford, 1969).
- ¹¹ Kenyon, D. H., and Steinman, G., *Biochemical Predestination* (McGraw-Hill, New York, 1969).
- ¹² Horowitz, N. H., Drake, F. D., Miller, S. L., Orgel, L. E., and Sagan, C., in *Biology and the Future of Man* (edit. by Handler, P.) (Oxford University Press, Oxford, 1970).
- ¹³ Barrett, A. H., *Proc. Conference on Interstellar Lines and Cosmochemistry* (New York Academy of Sciences, June 1971).
- ¹⁴ Solomon, P., *Proc. Conference on Interstellar Lines and Cosmochemistry* (New York Academy of Sciences, June 1971).
- ¹⁵ Littvak, M., *Proc. Conference on Interstellar Molecules and the Origin of Life* (NASA Ames Research Center, 1970).
- ¹⁶ Spitzer, L., in *Nebulae and Interstellar Matter* (edit. by Middlehurst, B. M., and Aller, L. H.) (University of Chicago Press, Chicago, 1968).
- ¹⁷ Townes, C., in *Highlights of Astronomy*, 2, 359 *Trans. Intern. Astron. Union* (D. Reidel, Dordrecht, 1971).
- ¹⁸ Sagan, C., and Miller, S. L., *Astron. J.*, 65, 499 (1960).
- ¹⁹ Sagan, C., Dayhoff, M. O., Lippincott, E. R., and Eck, R. V., *Nature*, 213, 273 (1967); Lippincott, E. R., Eck, R. V., Dayhoff, M. O., and Sagan, C., *Astrophys. J.*, 147, 753 (1967).
- ²⁰ Glasstone, S., Laidler, K. H., and Eyring, H., *The Theory of Rate Processes* (McGraw-Hill, New York, 1941).
- ²¹ Bar-Nun, A. and N., Bauer, S., and Sagan, C., *Science*, 168, 470 (1970).
- ²² *Origin of the Solar System* (edit. by Jastrow, R., and Cameron, A. G. W.) (Academic Press, New York, 1963).
- ²³ Urey, H. C., *Mon. Not. Roy. Astron. Soc.*, 131, 199 (1966).
- ²⁴ Cameron, A. G. W. (submitted to *Icarus*).
- ²⁵ *Stellar Evolution* (edit. by Stein, R. F., and Cameron, A. G. W.) (Plenum, New York, 1966).
- ²⁶ Ezer, D., and Cameron, A. G. W., *Canad. J. Phys.*, 45, 3429 (1967).
- ²⁷ Sagan, C., and Khare, B. N., *Science*, 173, 417 (1971).
- ²⁸ Studier, M. H., Hayatsu, R., and Anders, E., *Geochim. Cosmochim. Acta*, 32, 151 (1968).
- ²⁹ Herbig, G. H., in *Evolution Stellaire Avant le Sequence Principale*, *Proc. XVI Liege International Astrophysical Colloquium*, Royal Belgian Academy of Sciences, 1970; *Sterne und Weltraum*, 10, 4 (1971).
- ³⁰ Hoyle, F., Cameron, A. G. W., and Safronov, V. S., in *Highlights of Astronomy*, 2, 429 *Trans. Intern. Astron. Union* (D. Reidel, Dordrecht, 1971).
- ³¹ Salpeter, E. E., in *Highlights of Astronomy*, *Trans. Intern. Astron. Union*, 14, B (D. Reidel, Dordrecht, 1971).
- ³² Calvert, J. G., and Pitts, J. N., *Photochemistry* (Wiley, New York, 1966).
- ³³ Sagan, C., *Evolution*, 11, 40 (1957).
- ³⁴ Baldwin, B., and Schaeffer, Y., *J. Geophys. Res.*, 76, 4653 (1971).
- ³⁵ Sagan, C., in *The Origins of Prebiological Systems* (edit. by Fox, S. W.), 164 (Academic Press, New York, 1965).
- ³⁶ Ringwood, A. E., *J. Geophys. Res.*, 75, 6453 (1970).
- ³⁷ Wasserburg, G. J., *Proc. COSPAR Symposia, Working Group 7* (Seattle, Washington, June 1971).
- ³⁸ Gold, T., *Proc. Amer. Philos. Soc.*, 115, 74 (1971).

Anatomical and Ecological Evidence of Endothermy in Dinosaurs

ROBERT T. BAKKER

Department of Vertebrate Paleontology, Museum of Comparative Zoology, Harvard University, Cambridge, Massachusetts 02138

Recognition of endothermy in dinosaurs can explain both the success and the extinction of this group in the Mesozoic.

MOST birds and mammals are endotherms, producing at rest an order of magnitude more heat than a reptile of the same weight at the same temperature, and using high endogenous heat production to maintain a constant body temperature¹. Living reptiles are ectothermic and rely mostly on external heat sources for temperature regulation. Traditionally, dinosaurs are pictured as ectotherms², but this view has been challenged recently because of resemblances in the locomotor apparatus³⁻⁵ between dinosaurs and birds or mammals.

Anatomical Evidence

Fossil footprints and joint anatomy⁶ show that dinosaurs had a fully erect gait with a very narrow trackway, and limb action severely restricted to a vertical, parasagittal plane. Large, non-dinosaurian, fossil reptiles—pareiasaurs, tapinocephalians, rhynchosaurs—had a lizard-like, sprawling gait, very short and stocky limbs, and they must have been capable of only a low sustained speed⁵. Large dinosaurs—brontosaurus, duck-bills, stegosaurs, horned dinosaurs—in contrast had relatively much longer limbs, with joint anatomy similar to that of elephants or rhinos, and probably could reach speeds equal to those of large living elephants, that is 30 km h⁻¹ (ref. 6). Some small dinosaurs—ostrich dinosaurs, hypsilophodonts—had very long, gracile limbs and probably could reach top speeds similar to those in ostriches and ungulates, that is, 50–80 km h⁻¹. The cost of locomotion in mammals has been found⁷ to increase linearly with speed, and the slope of this relation decreases with body weight. This cost can be estimated by the formula

$$M = 8.46W^{-0.40} S + 1.7R$$

where M is ml. O₂ g⁻¹ h⁻¹ consumed, W is weight in g; S is speed in km h⁻¹, and R is resting metabolism in ml. O₂ g⁻¹ h⁻¹. Moberly⁸ has found for the lizard *Iguana*, and I have found for the lizards *Varanus*, *Tupinambis*, *Ctenosaura* and *Uromastix*, that the cost of locomotion in these reptiles is about the same as in mammals of the same weight. At a body temperature of 35° C the maximum sustained aerobic energy output of a lizard is about six times the resting level, or about $6W^{-0.25}$ ml. O₂ g⁻¹ h⁻¹, only about one-tenth to one-twentieth that of an endotherm^{1,9}. Except for very short (20 s or less) anaerobic bursts, the maximum speed of a 1 kg lizard at 30–35° C is about 1–2 km h⁻¹, about one-tenth to one-twentieth that of mammals of similar weight¹⁰. Maximum sustained speed for a lizard-like ectotherm can be

estimated by the formula

$$S = \frac{4.3W^{-0.25}}{8.46W^{-0.40}}$$

Assuming the presence of lizard-like energetics, the maximum speed of a 100 kg ostrich dinosaur would be only 2.9 km h⁻¹, and that of a 10 ton tyrannosaur only 5.8 km h⁻¹. Clearly, dinosaurs were built for sustained speeds much higher than these, and energy metabolism in these creatures must have been like that of endotherms.

In non-aquatic reptiles, the internal nares open near the front of the palate and can be blocked by mastication of food. Mammals have an extensive secondary palate which allows elaborate mastication while maintaining continuous breathing, a necessity for endotherms. In most dinosaurs the internal nares were displaced posteriorly more than in lizards. In herbivorous dinosaurs with specialized feeding apparatuses—sauropods, duck-bills, ceratopsians, ankylosaurs—the secondary palate was especially well developed, but it is absent in herbivorous lizards.

The heart and lungs of crocodilians are the most advanced and endotherm-like of living reptiles¹¹; there is a relatively great area for gaseous exchange and a complete anatomical separation of oxygenated and deoxygenated blood in the heart, and the aerobic energy output seems to reach a higher maximum than in most other reptiles. Yet the postcranial anatomy and the heart and lungs of crocodilians are more primitive than those of dinosaurs and reflect lower speeds and activity levels⁵.

Birds have an air-sac system connected to the lungs that penetrates deeply into the body. Air sacs permit unidirectional flow over gaseous exchange surfaces and much more efficient oxygen extraction than in mammals¹². In the ostrich, *Struthio camelus*, for example, air sacs allow extensive thermoregulatory panting without inducing alkalosis¹³. The vertebrae of brontosaurus and theropods had large cavities for air sacs which exited through large pneumatopores as in birds, and these dinosaurs, unlike living reptiles, probably had an avian-type lung with unidirectional flow.

Most dinosaur specimens represent animals with a live weight of 50 kg or more⁵, the few smaller specimens probably representing juveniles of genera with an adult weight of 30–50 kg⁵. Although microvertebrates and mouse-sized salamanders, lizards and mammals are very common in Jurassic and Cretaceous deposits, small dinosaurs are unknown⁵. Why did dinosaurs fail to exploit the ecological possibilities of small body size utilized so successfully by other groups?

Because large tropical endotherms have a low surface area to volume ratio, they can maintain a constant body temperature without dense superficial insulation. Elephants, rhinos, hippos, pigmy hippos, many suids and armadillos, for

example, are nearly hairless, even at birth, but very small endotherms require insulation because even relatively brief exposure to cool temperatures would strain heat production. Thus the only naked small mammals are subterranean forms, such as the naked mole rat, *Heterocephalus*, which avoid surface temperature fluctuations. Fossil skin impressions show that dinosaurs lacked any superficial insulation², and this factor, combined with endothermy, would have restricted dinosaurs to large body size. Small ectotherms allow body temperature to fall when environmental conditions are adverse to behavioural thermoregulation; most endotherms must maintain a constant body temperature.

The tetrapod skeleton provides reserves of calcium phosphate for internal chemical homeostasis. Endothermy places greater demands on the homeostatic capacity of bone because: (1) variations in energy metabolism are greater in endotherms—the difference between resting and maximum energy output is an order of magnitude greater than in ectotherms; (2) endotherms regulate their internal environment more closely and more continuously.

The labile fraction of bone is concentrated in the youngest layers of the most recently formed haversian systems¹⁸, and the maintenance of large reserves of this bone depends on the number and rapidity of reconstruction of haversian systems. Rapid mineral transfer from bone to plasma also depends on bone vascularization and the minimum distance from labile bone to capillary. Mineral transfer from intercellular fluids to plasma provides minute to minute adjustment; transfer from labile bone to intercellular fluids provides hour to hour adjustment¹³. In mammals the compact bone is richly vascularized and contains large numbers of rapidly reconstructed haversian systems¹⁴, but in living reptiles the compact bone contains few if any haversian systems with little reconstruction and is often completely avascular¹⁵. Similarly, the compacta in most fossil reptiles was usually poorly vascularized with little reconstruction¹⁵, though close-packed haversian systems with evidence of rapid reconstruction are known in the specialized mammal-like reptile *Kannemeyeria*, which may have resembled mammals in various physiological aspects¹⁵. The bone histology of theropods, some brontosaurus and ornithischians was also mammal-like¹⁵.

Currey has described¹⁶ in artiodactyls a bone architecture—laminar bone—which provides a more intimate association of bone cells and capillaries than does haversian bone. Laminar bone is unknown in living reptiles but was present in *Dinodontosaurus*, a close relative of *Kannemeyeria*¹⁶. Laminar bone even more densely vascularized than that of artiodactyls was present in some prosauropod and brontosaurus dinosaurs^{16,17}. All the dinosaurs which I investigated display densely haversian or laminar bone, or both.

Energy Budgets

In a field study of the Komodo dragon (*Varanus komodoensis*), a powerful predator, Auffenberg estimated that a 50 kg lizard kills a large mammal (pig or deer) or finds a large carcass about once a month¹⁸. Assuming the weight of the average prey carcass is about half that of the lizard, the Komodo dragon thus takes its own body weight in prey every 60 days. Cheetahs (*Acinonyx jubatus*) take their own weight in prey every 10.0 days¹⁹; and figures for lions (*Panthera leo*) and wild dogs (*Lycaon pictus*) are 7.7–9.1 and 6.7 days respectively²⁰. Endotherms thus take prey at a rate an order of magnitude higher than ectotherms relative to body weight.

Secondary productivity (SP) is the fixation of energy into tissue by a heterotrophic population²¹, and in a steady state population SP can be expressed as the weight or caloric equivalent of all individuals dying per year. In most ectothermic populations SP is 15–30% of the total energy assimilated per year²¹, but in endotherm SP is only 0.5–3.0% of assimilation. The SP to assimilation ratio is thus an order

of magnitude higher in ectotherms, but the ratio of assimilation to standing crop (SC) is an order of magnitude lower in ectotherms than in endotherms. Hence, the SP/SC ratio is similar in ectotherms and endotherms of the same size—in six of the seven small endotherms listed by Golley²¹, SP was 100–300% of SC, much the same range as in small lizards²². The SP of a prey population is the maximum crop available to carnivores. Because endothermic carnivores require prey at a rate an order of magnitude higher than that of ectotherms, the predator SC to prey SC ratio in an ectothermic predator/prey complex should be an order of magnitude higher than in an endothermic complex.

A tiger (*Panthera tigris*) needs to ingest a minimum of fifteen times its weight in prey a year in the wild²³ (because of loss to scavengers, twice this amount may have to be killed^{19,20}). The SP/SC ratio is 25–30%^{24,25} in ungulates of the size taken by tigers (deer and large antelope of 50–200 kg²⁶). Thus a prey SC thirty to sixty times the tiger SC is necessary to supply the minimum food needs of the tiger. Maximum predator SC/prey SC ratio in such a community would be 1.5–3%. SP/SC ratios and food requirements per unit weight decrease with increasing body size²⁶, and therefore, if prey and predator are about the same size, as in the tiger-deer community, predator SC/prey SC ratios should be fairly constant for any body size in a particular climate. In one area of Rhodesia, it has been noted that lions take ungulates totalling about 1% of their number²⁷ and probably 1.5–3% by biomass. In Ngorongoro Crater, Tanzania, figures of 25,000 ungulates to 400 hyaenas and 80 lions²⁸ have been noted—a predator SC/prey SC ratio also of about 2–3%. Schaller has reported that 140,000 kg of ungulates supports six or seven tigers, a leopard and some jackals²³—about 1% of the prey by biomass. The predator SC/prey SC of cold temperate mammal communities may be much less than 1%^{25,29}.

Predator/Prey Ratios

In the Early Pliocene mammal community analysed by Voorhies³⁰, the predator SC was about 3.9% of prey SC, excluding elephants (which are probably too large to be taken by any of the carnivores present) or 3% of prey SC including elephants (Fig. 1). Predator/prey ratios in the Pliocene community studied by Shotwell³¹ were similar. For an Oligocene mammal community Clark *et al.*³² obtained a predator SC/prey SC ranging from 2.7–8.1% for three facies, and 4.5% for the combined collections (Fig. 1). Thus in both living and fossil tropical and warm temperate mammal communities, predator SC/prey SC ratios range from 1–4.5%.

The oldest fully terrestrial tetrapod fauna known to date is from the *Tapinocephalus* Zone, mid-Permian of South Africa^{33–35}, where the fauna was mostly very primitive mammal-like reptiles and pareiasaurs, all of which were almost certainly ectothermic. Aquatic prey must have been unimportant, for fish and amphibians are rare. Pareiasaurs, large herbivores, are usually found articulated, in a standing posture in blue mudstone³³, a facies and mode of preservation which are virtually unknown for the rest of the fauna, indicating that pareiasaurs were ecologically separate. The ratio of number of carnivores to herbivores is 25%, much higher than in most mammalian faunas³⁸. Pareiasaurs, tapinocephalians, titanosaurs and anteosaurs were all about 1,000 kg. Because skull length/body length ratios are fairly constant in primitive mammal-like reptiles, body weight for the smaller genera can be estimated by comparing the cube of the skull length to the cube of the skull length of an anteosaur (taken as 1,000 kg). For therocephalians average skull length was about 350 mm³⁵; for gorgonopsians, 150 mm; for dicynodonts, 80 mm; and for anteosaurs, 720 mm³⁴. Carnivore SC/herbivore SC was about 19.2% (including pareiasaurs, see Fig. 1) and 29.8% (excluding pareiasaurs). Thus predator SC/prey SC ratios in this

ectothermic community were an order of magnitude greater than in mammalian communities.

Russell³⁶ has noted in Canadian Cretaceous deposits that field parties tend to leave behind many specimens of the commonest forms—duck-bills and ceratopsians—so that the number of duck-bill and ceratopsian specimens collected should be increased by 100% and 50% respectively to give an accurate picture of relative abundance. This correction has been incorporated in Fig. 1 and Table 1. Live weight has been estimated by using the square of the minimum shaft circumference of the limb bones—femur only for bipeds; humerus and femur for quadrupeds. Crested duck-bills with long forelimbs are treated as quadrupeds; flat-headed duck-bills with short forelimbs are treated as bipeds (Table 1). Square of minimum shaft circumference of humerus and femur of an adult *Rhinoceros unicornis*, live weight about 2,000 kg, was used as a standard (Table 1).

Carnivore SC/herbivore SC ratios for three successive Canadian Late Cretaceous rock units are 3.3%, 2.2% and 2.0% (Fig. 1; Table 2); for combined collections they are 2.9%. In the latest Cretaceous (Lance and Hell Creek, Upper Edmonton formations) predator/prey ratios are as low or lower; only a few tyrannosaur specimens are known³⁷, but hundreds of *Triceratops* and dozens of duck-bills have been discovered³⁸. In the Late Jurassic Morrison Formation, the carnivore SC (mostly *Allosaurus*) was but a few per cent of herbivore SC (mostly giant brontosaurs, with stegosaurs and camptosaurus)³⁹. High percentages of

Table 1 Calculated Average Adult Weights of Late Cretaceous Dinosaurs

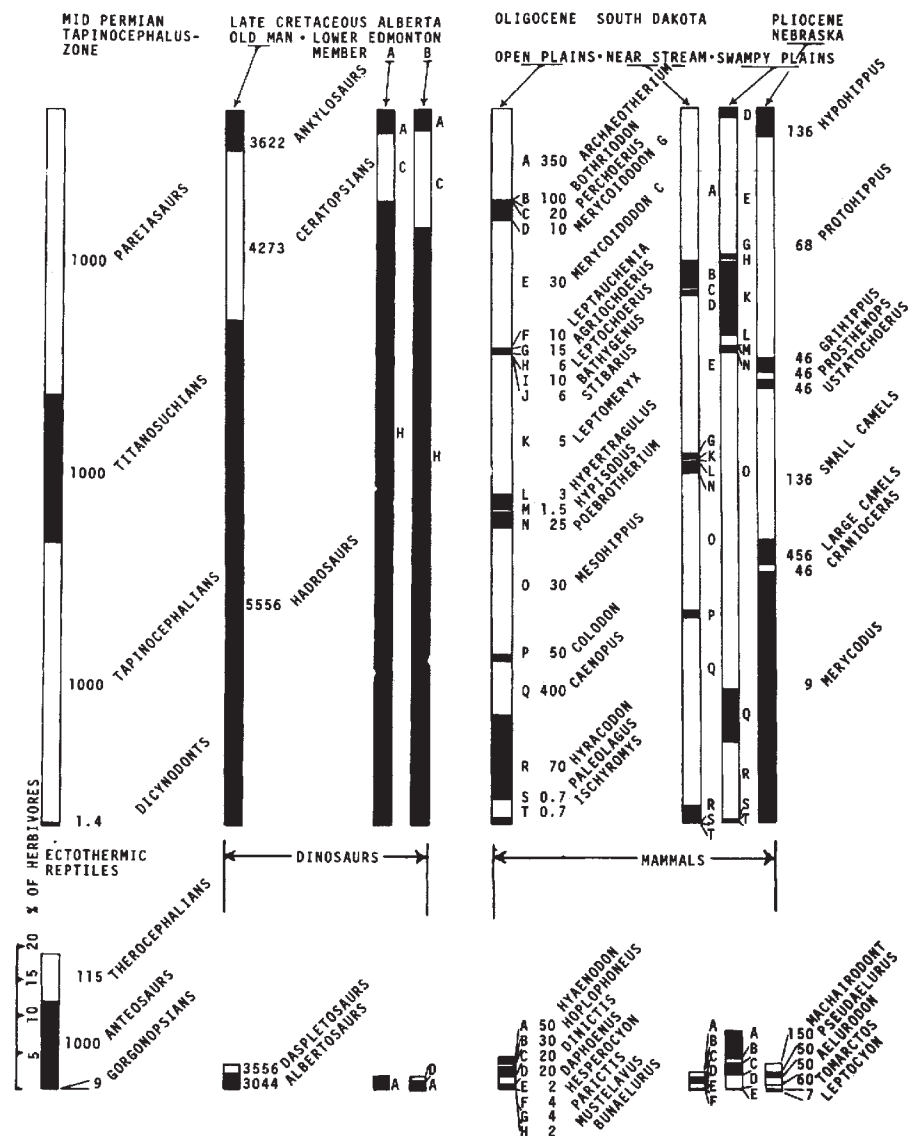
Animal	Square of minimum circumference of humerus in mm ²	Square of minimum circumference of femur in mm ²	Adult weight in kg	Caloric value of one specimen in A-kcal/m ² /yr
<i>Rhinoceros</i>	433	457	2,000	
<i>Albertosaurus</i>		1,355	3,044	1.0
<i>Daspletosaurus</i>		1,582	3,556	1.2
Hadrosaur, crested	500	2,020	5,556	1.8
Hadrosaur, flat-headed		2,520	5,556	1.8
Ceratopsians	900	1,001	4,273	1.4
Ankylosaurs	729	900	3,662	1.2

dinosaur carnivores occur only in obvious predator-trap deposits—local accumulations analogous to the tar pits of the California Pleistocene, such as the Cleveland-Lloyd Quarry where allosaurs of all age classes make up most of the fossils.

Energy Flow in Dinosaur Communities

An analysis of energy flow can determine whether the low percentage of carnivores was attributable to endothermy or some peculiarity of community structure—such as the

Fig. 1 Above: composition of herbivorous standing crop in fossil tetrapod communities. Numbers to right of column segments represent average adult weight for genus; height of column segment for each genus represents product of average weight and total number of specimens. Below: composition of carnivore standing crop for same fossil communities given above. Column height for herbivores and carnivores in each community drawn to same scale. Data from Boonstra³³, Russell³⁶, Clark *et al.*³² and Voorhies³⁰.



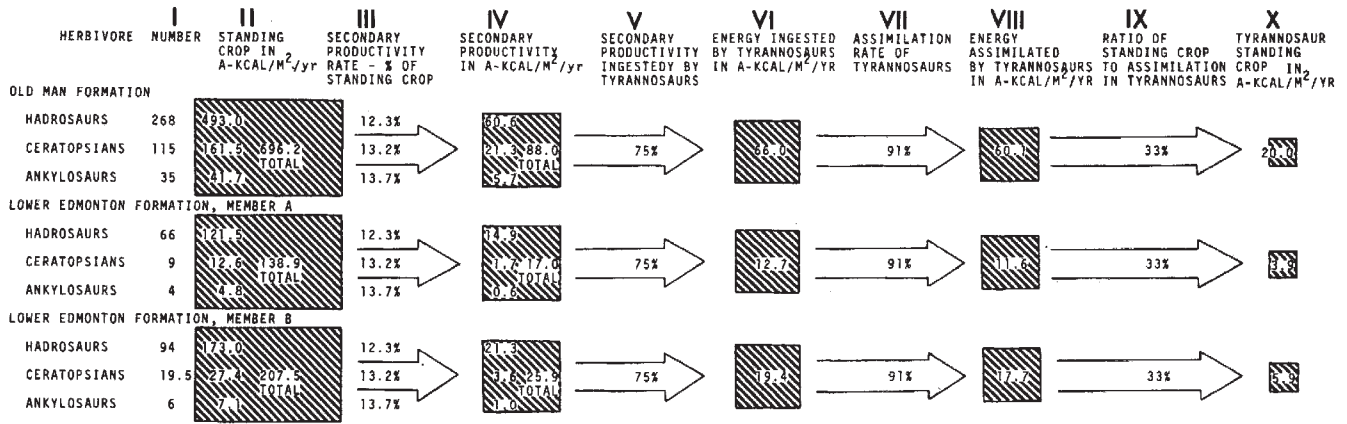


Fig. 2 Energy flow in Late Cretaceous dinosaur communities. Boxes represent energy quantities; arrows represent processes in energy transfer. Tyrannosaur standing crop (column 10) calculated from herbivore standing crop and assuming that dinosaurs were endothermic. Compare column 10 with Table 2.

large size of most common dinosaurs. Energy flow may be expressed by the equation: $SC_p = (SC_h)(SPR)(I)(D)(SC_p/A)$ where SC_p is the carnivore SC, SC_h is the herbivore SC, SPR is the secondary productivity rate of herbivores expressed as per cent of SC_h , I is per cent of herbivore SP ingested by predators, D is per cent of I assimilated by predators, and A is the total yearly assimilation (energy budget) of the predators. In most discussions of energy flow²¹, SC and SP are expressed in kilograms or kilocalories per acre or m². For dinosaur communities where such quantities cannot be computed, a relative unit is useful, and one A-kcal/m²/yr is defined as the caloric value of the standing crop of albertosaurs represented by one albertosaur specimen. The SC of any dinosaur thus becomes the number of specimens times the estimated body size divided by the estimated albertosaur body size (Table 2).

Environmental stress and severity of predation affect the SPR in living tetrapods. Musk oxen (*Ovibos moschatus*) in a barren ground Arctic habitat have lower SPR than warm temperate ungulates of comparable body size²⁰. African elephants (*Loxodonta africana*) suffer little predation⁴⁰ and have SPR rates of only about 5%²¹. The flora of dinosaur communities indicates a mild, frost-free climate⁴¹. Predation on even the largest Canadian dinosaur herbivores was probably very heavy. Solitary predators with relatively small heads—cheetah¹⁹, *Sceloporus*²²—usually take individual prey half to a fifth their body size or smaller. Big-headed predators—lions, tigers^{23,42-44}, Komodo dragons¹⁸—frequently take prey two or even three times their body weight. Duck-bills, the largest herbivores in the Cretaceous Canadian dinosaur communities, were less than twice the adult weight of the contemporary tyrannosaurs, *Albertosaurus* and *Daspletosaurus* (Table 1)⁴⁵. Tyrannosaurs had relatively huge heads (1,000 mm⁴⁶) and teeth and undoubtedly could kill even the largest contemporary herbivore. The SPR rate in tropical and temperate ungulates, which are heavily preyed upon, decreases with increasing body size, much as metabolism per gram decreases as the -0.25 power of body weight². The SPR of moose (*Alces alces*) in the moose-wolf com-

munity at Isle Royale is about 25%²⁴; in smaller white tail deer (*Odocoileus virginianus*)²⁵ and in tropical ungulates⁴² it is about 33%.

Dinosaur SPR may be estimated by the formula:

$$SPR = 0.22 \left(\frac{W}{550} \right)^{-2.50}$$

where 0.22 is a conservative estimate of SP/SC in temperate or tropical herbivores of the size of moose, 550 is the weight of an adult moose in kilograms, and W is the adult weight of the dinosaurs (Fig. 2, columns 2 and 4). Tyrannosaurs were the only carnivores powerful enough to kill and dismember duck-bills, horned dinosaurs and ankylosaurs. The next largest carnivores, *Stenonychosaurus* and *dromaeosaurs*^{46,47}, were only about 50 kg adult weight. Tyrannosaurs were long-legged, gracile, had good olfaction, and must have been the dominant scavengers and predators. At Isle Royale, moose (*Canis lupus*) are the dominant prey of wolves and, according to Jordan²⁴, ingest about 75+ % of the caloric value of all moose SP. Hence I, the ingestion of herbivore SP by tyrannosaurs, is estimated to be 75% (Fig. 2, columns 5 and 6). In bobcats (*Lynx rufus*), domestic cats and dogs⁴⁸, and the predatory lizard *Lacerta vivipara*⁴⁹, about 91% of ingested prey is assimilated. Tyrannosaur digestive efficiency, D, is estimated to be 91% (Fig. 2, column 7).

Low mortality rates decrease the SPR and the ratio between SC and assimilation (energy budget). Large carnivores, such as wolves, have lower mortality rates and SPR than most prey species of similar size^{24,25}. Tyrannosaur mortality was probably low, much as in living elephants which are about the same size and suffer little predation. Mortality of African elephants is about 4-5% per year and the SC/assimilation ratio is about 33%^{21,50}. If tyrannosaurs were endothermic with energy budgets like those of large, elephant-size mammals, SC/assimilation ratios were probably about 33% (Fig. 2, column 9). The SC_p of endothermic tyrannosaurs that should be supported by the prey SC_h can be estimated by taking 33% of the estimated assimilated prey SP, which has been calculated directly from herbivorous

Table 2 Standing Crop of Tyrannosaurs calculated from Tyrannosaur Fossils and from the Energy Flow Equation

Formation	Daspletosaurus specimens	Albertosaurus specimens	Tyrannosaur SC as % of herbivore SC	Tyrannosaur SC in A-kcal/m ² /yr	Endothermic tyrannosaur SC calculated from energy flow in A-kcal/m ² /yr	Ectothermic tyrannosaur SC calculated from energy flow in A-kcal/m ² /yr
Old Man	6	16	3.3	23.0	20.0	Approx. 200.0
Lower Edmonton Member A	0	3	2.2	3.0	3.9	Approx. 39.0
Lower Edmonton Member B	1	3	2.0	4.2	5.9	Approx. 59.0
Total	7	22	2.9	30.2	29.8	Approx. 298.0

dinosaur specimens (Fig. 2, columns 1 to 8). In ectotherms the SC/assimilation ratio is an order of magnitude higher than in endotherms. Thus if tyrannosaurs were ectothermic, the SC_p represented by the tyrannosaur specimens should be an order of magnitude greater than 33% of the calculated tyrannosaur energy budget.

The SC_p of endothermic tyrannosaurs that could be supported by the herbivorous dinosaur populations is 20.0 A-kcal/m²/yr for the Old Man Formation; 3.9 A-kcal/m²/yr for Member A, Lower Edmonton Formation; 5.9 A-kcal/m²/yr for Member B, Lower Edmonton Formation; and 29.8 A-kcal/m²/yr for all three (Fig. 2, column 10). If tyrannosaurs were ectothermic, these values should be an order of magnitude higher, that is, about 200.0 A-kcal/m²/yr for the Old Man Formation and about 300.0 A-kcal/m²/yr for all three formations. The actual caloric value represented by the tyrannosaur specimens (Table 2) is: 23.0 A-kcal/m²/yr for the Old Man; 3.0 A-kcal/m²/yr for Member A, Lower Edmonton; and 4.2 A-kcal/m²/yr for Member B, a total of 30.2 A-kcal/m²/yr. Thus the number of tyrannosaur specimens agrees very well with the calculated number of tyrannosaur-size endothermic predators which could be supported by the herbivorous dinosaur populations.

The predator SC/prey SC ratios in dinosaur communities are an order of magnitude less than in the all-ectothermic *Tapinocephalus* Zone fauna but fall within the range of living and fossil mammal communities. Analysis of energy flow strongly indicates that dinosaur energy budgets were like those of large mammals, not elephant-size lizards.

Dinosaur Success and Extinction

Abundant evidence for endothermy in dinosaurs comes from the three levels of biological organization which can be investigated through fossils—cellular (bone histology), gross anatomical (locomotor anatomy, pneumatopores, secondary palate), and community structure (low predator/prey ratios). On all these levels dinosaurs resemble advanced mammals or birds, not living reptiles. Recognition of endothermy in dinosaurs can explain both the success and the extinction of this group. The mammal-like reptiles which dominated tetrapod communities in the Permo-Triassic approached true mammals in many skeletal and presumably physiological characters, but even advanced mammal-like reptiles retained the primitive sprawling posture⁵. The most primitive living mammals (monotremes, didelphids, tenrecs, *Solenodon*) tend to regulate their body temperatures at relatively low levels (27–33°C) and have poor mechanisms for coping with either endogenous or environmentally induced heat stress⁵. Advanced mammal-like reptiles may have been similar. The locomotion, respiration and thermoregulation of dinosaurs probably resembled those of large ground birds or savannah ungulates and carnivores. Thus dinosaurs had an advantage over mammal-like reptiles in mobility and the capacity to unload high endogenous heat production during sustained, vigorous, daylight activity in a warm climate⁵.

Some microfossil evidence indicates that a sudden drop in world-wide temperature occurred at the Cretaceous-Tertiary boundary^{51,52}. Land tetrapod extinctions at the end of the Cretaceous were selective: many large and small ectotherms survived—salamanders, several varieties of frogs, champsosaurs, lizards, crocodilians and turtles; and small endotherms did not suffer total annihilation—many lineages of mammals and birds continued into the early Tertiary. Dinosaurs were totally wiped out. As L. S. Russell has suggested³, the combination of large size, naked skin and endothermy may have caused the extinction of dinosaurs. In the face of sudden, prolonged cold stress, dinosaurs were too large to escape by hibernating in burrows or other micro-habitats available to small endotherms, and were unable to survive prolonged drops in their body temperature, unlike many

turtles, lizards and other ectotherms. Exposure to prolonged severe cold would probably kill most of the living, naked, tropical endotherms—rhinos, hippos, elephants, armadillos—and may well have eliminated the dinosaurs, the naked, tropical endotherms of the Mesozoic.

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- ¹ Gordon, M. S., Bartholomew, G. A., Grinnell, A. D., Jorgensen, C. B., and White, F. N., *Animal Function: Principles and Adaptations* (Macmillan, New York, 1968).
- ² Colbert, E. H., *Dinosaurs: Their Discovery and Their World* (Dutton, New York, 1961).
- ³ Russell, L. S., *J. Paleont.*, **39**, 497 (1965).
- ⁴ Ostrom, J. H., *Proc. N. Amer. Paleont. Conf.*, **D**, 347 (1969).
- ⁵ Bakker, R. T., *Evolution*, **25**, 636 (1971).
- ⁶ Howell, A. B., *Speed in Animals* (Univ. Chicago Press, Chicago, 1944).
- ⁷ Taylor, C. R., Schmidt-Nielsen, K., and Raab, J. L., *Amer. J. Physiol.*, **219**, 1104 (1970).
- ⁸ Moberly, W. R., *Comp. Biochem. Physiol.*, **27**, 21 (1968).
- ⁹ Jansky, L., *Acta Univ. Carolinae, Biologica*, **1** (1965).
- ¹⁰ Layne, J. N., and Benton, A. N., *J. Mamm.*, **35**, 103 (1954).
- ¹¹ Waterman, A. J., *Chordate Structure and Function* (Macmillan, New York, 1971).
- ¹² Schmidt-Nielsen, K., Kanwisher, J., Lasiewski, R. C., Cohn, J. E., and Bretz, W. L., *Condor*, **71**, 341 (1969).
- ¹³ McLean, F. C., and Urist, M. R., *Bone* (Univ. Chicago Press, Chicago, 1968).
- ¹⁴ Enlow, D. H., and Brown, S. O., *Texas J. Sci.*, **3**, 187 (1958).
- ¹⁵ Enlow, D. H., and Brown, S. O., *Texas J. Sci.*, **2**, 186 (1957).
- ¹⁶ Currey, J. D., *Palaeontology*, **5**, 238 (1962).
- ¹⁷ Tarlo, L. B., and Mercer, J. R., *Sixteenth Symp. Vert. Palaeont. Comp. Anat. (Reading)* (1968).
- ¹⁸ Auffenberg, W., *Animal Kingdom*, **73**, 19 (1971).
- ¹⁹ Schaller, G. B., *E. Afr. Wildl. J.*, **6**, 95 (1968).
- ²⁰ Wright, B. S., *J. Wildl. Mgmt*, **24**, 1 (1960).
- ²¹ Golley, F. B., *Amer. Zool.*, **8**, 53 (1968).
- ²² Blair, W. F., *The Rusty Lizard* (Univ. Texas Press, Austin, 1960).
- ²³ Schaller, G. B., *The Deer and the Tiger* (Univ. Chicago Press, Chicago, 1967).
- ²⁴ Jordan, P. A., Botkin, D. B., and Wolfe, M. L., *Ecology*, **52**, 147 (1971).
- ²⁵ Wagner, F. H., in *Ecosystem Concepts in Natural Resource Management* (edit. by Van Dyne, G. M.), 284 (Academic Press, New York, 1969).
- ²⁶ Kleiber, M., *The Fire of Life* (Wiley, New York, 1961).
- ²⁷ Dasmann, R. F., and Mossman, A. S., *J. Wildl. Mgmt*, **26**, 262 (1962).
- ²⁸ Myers, N., *Intern. Wildl.*, **2**, 34 (1971).
- ²⁹ Hornocker, M. G., *Wildl. Monog.*, **21** (1970).
- ³⁰ Voorhies, M. R., *Contr. Geol. Spec. Paper, Univ. Wyoming*, **1** (1969).
- ³¹ Shotwell, J. A., *Ecology*, **36**, 332 (1955).
- ³² Clark, J., Beerbower, J. R., and Kietze, K. K., *Fieldiana, Geol. Mem.*, **5** (1967).
- ³³ Boonstra, L. D., *Ann. S. Afr. Mus.*, **56** (1969).
- ³⁴ Boonstra, L. D., *Ann. S. Afr. Mus.*, **42**, 108 (1954).
- ³⁵ Boonstra, L. D., *Ann. S. Afr. Mus.*, **42**, 65 (1954).
- ³⁶ Russell, D. A., *Nat. Hist. Pap. Nat. Mus. Canada*, **36** (1967).
- ³⁷ Osborn, H. F., *Bull. Amer. Mus. Nat. Hist.*, **35**, 733 (1917).
- ³⁸ Ostrom, J. H., and McIntosh, J. S., *Marsh's Dinosaurs* (Yale Univ. Press, New Haven, 1966).
- ³⁹ Freeman, M. M. R., *J. Wildl. Mgmt*, **35**, 103 (1971).
- ⁴⁰ Sikes, S. K., *The Natural History of the African Elephant* (American Elsevier, New York, 1971).
- ⁴¹ Bell, W. A., *Bull. Canad. Geol. Survey*, **13** (1949).
- ⁴² Kruuk, H., and Turner, M., *Mammalia*, **31**, 1 (1967).
- ⁴³ Hirst, S. M., *Zool. Afr.*, **4**, 199 (1969).
- ⁴⁴ Mitchell, B. L., Shenton, J. B., and Uys, J. C., *Zool. Afr.*, **1**, 297 (1965).
- ⁴⁵ Russell, D. A., *Nat. Sci. Pub. Paleont. Nat. Mus. Canada*, **1** (1970).
- ⁴⁶ Colbert, E. H., and Russell, D. A., *Novitates*, 2380 (1969).
- ⁴⁷ Russell, D. A., *Canad. J. Earth Sci.*, **6**, 595 (1969).
- ⁴⁸ Golley, F. B., Petrides, G. A., Rauber, E. L., and Jenkins, J. H., *J. Wildl. Mgmt*, **29**, 442 (1965).
- ⁴⁹ Avery, R. A., *J. Anim. Ecol.*, **40**, 351 (1971).
- ⁵⁰ Laws, R. M., and Parker, I. S., in *Comparative Nutrition of Wild Animals* (edit. by Crawford, M. A.), 319 (Academic Press, London, 1968).
- ⁵¹ Worsley, T. R., *Nature*, **230**, 318 (1971).
- ⁵² Russell, D. A., *Canad. Geog. J.*, **83**, 204 (1971).

The Mass Difference of the Muon and the Electron

F. HOYLE & J. V. NARLIKAR

Institute of Theoretical Astronomy, University of Cambridge

The fact that the muon and the electron have different masses can be understood in terms of a Machian theory of inertia. The masses of particles are not fixed and immutable but depend on the detailed particle composition of the universe.

THE mass difference of the electron and muon poses one of the inscrutable problems of particle physics. Except for the mass difference the muon does not appear to be any different from the electron. Its electromagnetic properties are deduced from those of the electron simply by changing the masses in the appropriate formulae. Like the electron it does not appear to experience strong interactions. Why should nature thus have two particles identical in all respects except for mass? Leaving this problem unanswered does not put any obstacle in the way of an experimental study of elementary particles. Nor does it seem to disturb current theoretical ideas; one can just accept the mass difference, and proceed from there. Most theoreticians would regard such a state of affairs as unsatisfactory, however.

An understanding of the muon-electron mass difference requires the electron e and the muon μ to be parts of some structure. It is natural to include the electron neutrino ν_e and the muon neutrino ν_μ in this structure, thereby involving all the leptons. It is interesting that the experimentally determined properties of weak decays support the concept of structure within the lepton family. We may refer to the pair e, ν_e as "electrons", and to the pair μ, ν_μ as "muons", the antiparticles being $\bar{e}, \bar{\nu}_e, \bar{\mu}, \bar{\nu}_\mu$. With the convention that particles count positively and antiparticles negatively it is found that electrons and muons are separately conserved in all reactions. For example, both sides in the weak decay

$$\mu \rightarrow e + \bar{\nu}_e + \nu_\mu \quad (1)$$

have a muon number of unity and an electron number of zero. This separate conservation of "muons" and "electrons" supports the idea that a structural relationship should be built from the pairs (e, ν_e) , (μ, ν_μ) . The available structures seem very simple, however; too simple to make the muon-electron mass difference seem at first sight to be a tractable problem. In an obvious notation why is $m_e \ll m_\mu$? Why is $m_{\bar{e}} \ll m_e$? Why is $m_{\bar{\mu}} \gg m_\mu$? Why are neutrinos uncharged?

We argue here that lack of progress in answering all these questions is due to the physicist's implicit belief that particle masses are of local origin. If one takes the view that particle masses can be of cosmological origin then a new set of ideas is brought into play. In particular, all the above questions concerning leptons can be answered in a natural and elegant way.

Theoretical Framework

The Newtonian concept that inertia is an intrinsic property of matter is described in modern classical theory by the action term

$$-m_a \int da \quad (2)$$

Here da is an element of proper time along the world line of

particle a and m_a is its mass. In the usual theory m_a is specified once and for all. In a Machian theory, on the other hand, m_a is not wholly intrinsic to the particle, and is not necessarily a constant, since the value of m_a depends not only on particle a but also on the background provided by the rest of the matter in the universe. This idea has been developed in various ways by different authors. We shall confine ourselves here to the line of attack adopted in our earlier work¹. We rewrite equation (2) as

$$-\int m_a(A) da \quad (3)$$

with

$$m_a(A) = \sum_{b \neq a} \int \tilde{G}(A, B) db \quad (4)$$

thereby giving quantitative form to Mach's idea. The function $\tilde{G}(A, B)$ is a propagator conveying inertia that goes from B on the world line of particle b to point A on the world line of particle a . The sum over all $b \neq a$ gives the total effect of the "rest of the universe" on particle a . The propagator $\tilde{G}(A, B)$ is symmetric between points A and B

$$\tilde{G}(A, B) = \tilde{G}(B, A) \quad (5)$$

and satisfies the wave equation

$$\square_X \tilde{G}(X, B) + \frac{1}{6} R(X) \tilde{G}(X, B) = [-g(X)]^{-1/2} \delta_4(X, B) \quad (6)$$

in a Riemannian space of scalar curvature R and of metric g_{ik} (g is the determinant of g_{ik}). In flat space

$$\tilde{G}(A, B) = \frac{1}{4\pi} \delta[(a_i - b_i)(a^i - b^i)] = \frac{1}{4\pi} \delta(s_{AB}^2) \quad (7)$$

where $a^i, b^i, i = 1, 2, 3, 4$, are the coordinates of points A, B and s_{AB}^2 is the square of the 4-dimensional distance between them. The usual notation for the Dirac delta functions is used in equations (6) and (7). The inertial action is now a many-particle one and is written in the form

$$-\sum_{a < b} \iint \tilde{G}(A, B) da db \quad (8)$$

Earlier work¹ gave cogent reasons for arriving at this description and also stressed the uniqueness of the approach.

The above picture is a classical one, but can readily be converted into a non-relativistic quantum theory by using Feynman's path integral technique². Conversion into relativistic quantum theory is more difficult but more illuminating in the sense that it leads to new ideas and new results. In particular, we have to change equation (8) to include spin, in such a way that the particle in question satisfies the Dirac equation with mass given by equation (4). The action is changed to

$$-\sum_{a < b} \iint \tilde{G}(A, B) da^* db^* \quad (9)$$

where $da^* = \gamma_i da^i$, $db^* = \gamma_i db^i$; da^i, db^i being coordinate displacements along particular paths of particles a and b , and γ_i being the usual 4×4 gamma matrices. At first sight equation (9) might seem to make m_a a spinor, but averaging with respect to all paths of particle b in fact makes m_a a scalar. The result of this averaging is expressed by

$$\langle \int \tilde{G}(A, B) db^* \rangle = \int \tilde{G}(A, B) \sqrt{-g(B)} [\bar{\psi}^{(b)} \psi^{(b)}]_B da^* db \quad (10)$$

where $\psi^{(b)}$ is the wave function of particle b . We can write

$$\psi^{(b)} = \begin{bmatrix} u_a \\ v^b \end{bmatrix} \quad (11)$$

u_a, v^b being two-component spinors making up the Dirac 4-spinor $\psi^{(b)}$. Then $\bar{\psi}^{(b)}\psi^{(b)}$ is the scalar

$$\bar{\psi}^{(b)}\psi^{(b)} = u_a v^a + u_{\dot{b}} v^{\dot{b}} \quad (12)$$

How this approach leads to the Dirac equation is described in an earlier paper³ and will not be repeated here.

With this background we turn to the electron-muon problem. We begin by considering an abstract spin space M of two-component "spinors" K_Λ which transform according to

$$K'_\Lambda = Q_\Lambda^\Sigma K_\Sigma, \Lambda, \Sigma = 1, 2 \quad (13)$$

where Q_Λ^Σ is a complex unimodular matrix (the considerations of the present article do not require any further restriction on Q , although further work may well require Q to be unitary). The elements of the product space $M \times M$ transform according to

$$L'_{\Gamma\Delta} = Q_\Gamma^\Lambda Q_\Delta^\Sigma L_{\Lambda\Sigma} \quad (14)$$

and in general are complex. The indices can be raised and lowered in the usual way by the fundamental spinors $\epsilon^{\Gamma\Delta}$ and $\epsilon_{\Gamma\Delta}$ where

$$\epsilon_{12} = -\epsilon_{21} = \epsilon^{12} = -\epsilon^{21} = 1, \quad \epsilon_{11} = \epsilon_{22} = \epsilon^{11} = \epsilon^{22} = 0 \quad (15)$$

Thus we have

$$J^\Lambda = \epsilon^{\Lambda\Gamma} J_\Gamma, \quad J_\Lambda = \epsilon_{\Lambda\Gamma} J^\Gamma \quad (16)$$

We can arrange the four leptons e, ν_e, μ, ν_μ , each represented by a normal Dirac 4-spinor, into a matrix $L_{\Gamma\Lambda}$,

$$L_{\Gamma\Lambda} \equiv \begin{bmatrix} \nu_e & e \\ -\mu & \nu_\mu \end{bmatrix} \quad (17)$$

the minus sign in L_{21} being used for convenience, as will become clear later. From equations (15) and (16) we have

$$L^{\Gamma\Lambda} = \begin{bmatrix} \nu_\mu & \mu \\ -e & \nu_e \end{bmatrix} \quad (18)$$

We now make the postulate that the mass interactions for leptons, and also the electromagnetic interactions, must be invariant with respect to the transformations Q in our M space. This implies that these interactions should contain the matrix L in an invariant form.

For the mass interaction we now take

$$- \sum_{a < b} \iint \tilde{G}(A, B) L^{(a)}_{\Gamma\Lambda} L^{(b)\Gamma\Lambda} da^* db^* \quad (19)$$

in place of equation (9). From equations (17) and (18) we have

$$L^{(a)}_{\Gamma\Lambda} L^{(b)\Gamma\Lambda} = L^{(a)\Gamma\Lambda} L^{(b)}_{\Gamma\Lambda} = v_e^{(a)} v_\mu^{(b)} + v_\mu^{(a)} v_e^{(b)} + e^{(a)} \mu^{(b)} + \mu^{(a)} e^{(b)} \quad (20)$$

It is seen that m_e arises from interaction with μ leptons, m_μ arises from interaction with e leptons, m_{ν_e} from ν_μ , and m_{ν_μ} from ν_e . Thus

$$m_e(A) = \sum_\mu \iint \tilde{G}(A, B) [\bar{\psi}_\mu \psi_\mu]_B \sqrt{-g(B)} d^4 b \quad (21)$$

the summation being over all μ leptons. Similarly, we have

$$m_\mu(A) = \sum_e \iint \tilde{G}(A, B) [\bar{\psi}_e \psi_e]_B \sqrt{-g(B)} d^4 b \quad (22)$$

$$m_{\nu_\mu}(A) = \sum_{\nu_\mu} \iint \tilde{G}(A, B) [\bar{\psi}_{\nu_\mu} \psi_{\nu_\mu}]_B \sqrt{-g(B)} d^4 b \quad (23)$$

$$m_{\nu_e}(A) = \sum_{\nu_e} \iint \tilde{G}(A, B) [\bar{\psi}_{\nu_e} \psi_{\nu_e}]_B \sqrt{-g(B)} d^4 b \quad (24)$$

If a mass coupling constant, λ^2 say, were introduced into equation (21), the same λ^2 should be included in equations (22), (23), (24).

To extend these ideas to electrodynamics we note that in former work the electromagnetic interaction in flat space

between charges e_a and e_b was taken to be

$$e_a e_b \iint \delta(s^2_{AB}) \eta_{ik} da^i db^k \quad (25)$$

where η_{ik} is the Minkowski tensor. We now modify equation (25) by introducing L matrices. Writing $e_a = e_b = e$ (the electronic charge) we replace equation (25) by

$$e^2 \iint L^{(a)}_{\Gamma\Gamma} \delta(s^2_{AB}) L^{(b)}_{\Lambda\Lambda} \eta_{ik} da^i db^k \quad (26)$$

From equation (17), together with the rule for raising suffices, we easily obtain

$$L_{\Gamma\Gamma} = \mu + e \quad (27)$$

Hence equation (26) becomes

$$e^2 \iint [\mu^{(a)} + e^{(a)}] \delta(s^2_{AB}) [\mu^{(b)} + e^{(b)}] \eta_{ik} da^i db^k \quad (28)$$

telling us that μ and e interact among each other with the same charge. The neutrinos ν_e, ν_μ are absent from equation (28).

Answers to Questions on Leptons

The questions raised above in the introductory section were, first, why do ν_e, ν_μ not interact electromagnetically? Second, why are m_{ν_e}, m_{ν_μ} either zero or very small? Third, why is m_μ large compared with m_e ?

The answer to the first of these questions was given already at the end of the preceding section. When the invariants $L^{(a)}_{\Gamma\Gamma}, L^{(b)}_{\Lambda\Lambda}$ are included in (26) only μ and e enter the electromagnetic interaction.

The second question can be dealt with in terms of a self-consistent loop of argument. Starting with $m_{\nu_e} = 0, m_{\nu_\mu} = 0$, the Dirac equation for either the ν_e or ν_μ leptons (compare equation (11)) separates into two pairs, one pair for u_a , the other for v^b . It is possible therefore to set either u_a or v^b zero, retaining only a single two-component spinor to represent each of these particles. According to equation (12) we then have

$$\bar{\psi}_{\nu_e} \psi_{\nu_e} = \bar{\psi}_{\nu_\mu} \psi_{\nu_\mu} = 0 \quad (29)$$

in which case equations (23) and (24) lead to $m_{\nu_e} = 0, m_{\nu_\mu} = 0$, thereby completing the loop.

Is there a similar loop for e and μ ? Not if the electromagnetic interaction contributes self-terms to m_e, m_μ , since the Dirac equation for e and for μ is not then separable with respect to the spinors u_a, v^b . The scalar quantities $\bar{\psi}_e \psi_e, \bar{\psi}_\mu \psi_\mu$ are not in general zero, so that equations (21) and (22) lead to further contributions to m_e, m_μ .

The present theory is symmetric with respect to e and μ . How then can we have $m_e \neq m_\mu$? If the universe were symmetric in its content with respect to e and μ we should have $m_e = m_\mu$, but such a situation is unstable because of weak decays. Suppose a fluctuation in which one of the charged leptons becomes somewhat more abundant than the other. Call e the more abundant. Then the cosmological contribution to m_e will be slightly less than the contribution to m_μ . Weak decays proceed in the sense of equation (1), from μ to e , which increases the fluctuation. The answer to the third of the above questions lies therefore in the instability with respect to weak decay of a symmetric universe. We have $m_\mu \gg m_e$ because there are many more electrons than muons in the world.

The available experimental data set m_μ as about 207 m_e . Since astrophysical considerations make it seem likely that the abundance ratio of e to μ in the world is greater than 207, it is natural to argue that m_e is largely a self electromagnetic mass, as has often been suggested before.

The main idea of this article, that the masses of particles are related to the content of the universe, is capable of extension. It may well be the case that while much of the logical structure of particle physics can be described within a purely local theory, the explicit values for masses and even for coupling constants have to be determined in relation to the universe as a whole.

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¹ Hoyle, F., and Narlikar, J. V., *Proc. Roy. Soc., A*, **294**, 138 (1966).

² Feynman, R. P., *Rev. Mod. Phys.*, **20**, 367 (1948).

³ Hoyle, F., and Narlikar, J. V., *Il Nuovo Cimento*, **7A**, 242 (1972).

LETTERS TO NATURE

PHYSICAL SCIENCES

The Luminosity-Spectral Index Relationship for Radio Galaxies

THE relationship between spectral index and luminosity for the 3CR extragalactic sources suggests¹⁻³ that sources with high radio luminosity have, on the average, steeper spectra than do the low luminosity sources. But as Kellermann *et al.*³ point out, the scatter in the relationship is quite large. In addition, they found that the correlation exists only for radio galaxies and not for quasars.

We show here that even for radio galaxies this relationship holds only for a particular subgroup in the 3CR catalogue. For those galaxies with power law spectra there is a clear relationship between luminosity and spectral index, but for those galaxies which do not have simple power law spectra, luminosity is independent of spectral index.

The spectral classifications and spectral indices we have used are taken from a study of 3CR spectra at frequencies between 10 MHz and 10,700 MHz (our unpublished results). Most of the radio galaxies belong to either class S or class C⁻. Class S sources are those whose spectra are well represented by a straight line in the log flux density—log frequency plane, that is, flux

density $\propto (\text{freq})^{-x}$, where x is the spectral index. Class C⁻ sources are those whose spectra are well represented by two intersecting straight lines, the line at high frequencies being steeper than the one at low frequencies. The break frequency where the slope changes is not fixed, but is determined separately for each source.

We find that, unlike the radio galaxies in general, the majority of N galaxies and Seyfert galaxies do not fit into these two spectral classes. Because of this, and because of their strong resemblance to quasi-stellar sources⁴, we did not include N galaxies and Seyfert galaxies in the luminosity-spectral index plots (Fig. 1).

The monochromatic luminosities of the galaxies are required in constructing a luminosity-spectral index plot. Published redshifts are available for less than half of these objects, however, and with few exceptions those galaxies which have measured redshifts are brighter than about 17 mag. In order to include the fainter galaxies, and thus avoid a possible selection effect which might introduce a false correlation, we have estimated redshifts for fainter galaxies from their optical magnitudes and constructed a Hubble diagram from radio galaxies with measured redshifts. Although this process has unavoidably introduced some additional scatter into the luminosities, we believe it preferable to risking a possible selection effect. Monochromatic luminosities at 178 MHz were

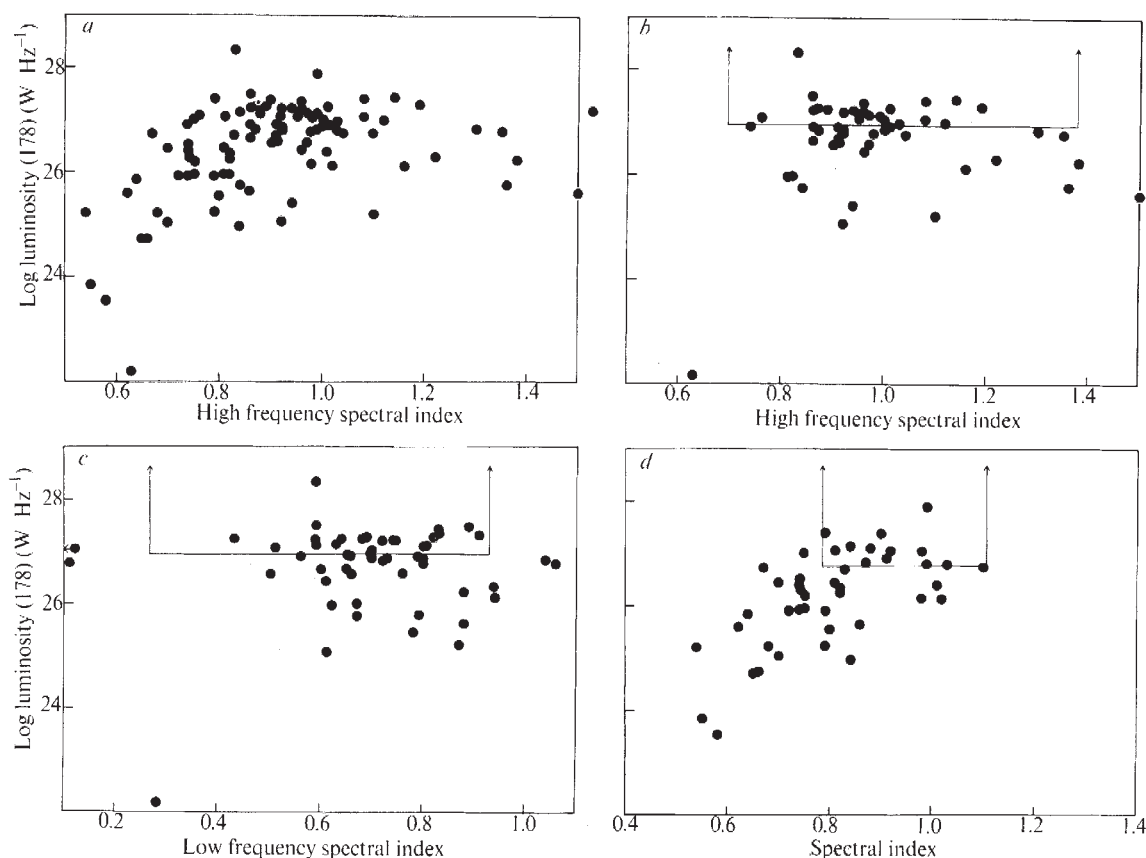


Fig. 1 Luminosity against spectral index for 3CR radio galaxies. *a*, All 3CR galaxies—high frequency spectral index; *b*, class C⁻ galaxies—high frequency spectral index; *c*, class C⁻ galaxies—low frequency spectral index; *d*, class S galaxies.

then calculated from the flux densities and redshifts, using the Einstein-de Sitter cosmological model.

Fig. 1a is a plot of luminosity at 178 MHz against spectral index for all the radio galaxies together, comprising 44 class S galaxies and 55 class C⁻ galaxies. For the class C⁻ galaxies, we used the high frequency spectral index. A relationship between luminosity and spectral index, similar to that given in Table 7 of Kellermann *et al.*³, can be derived from these data, but it is clear that the scatter in Fig. 1a is very large, and the relationship is not well defined.

We now divide the data into two groups, the class S galaxies and the class C⁻ galaxies. Fig. 1b is a plot of luminosity at 178 MHz against high frequency spectral index for the class C⁻ galaxies only. Fig. 1c is a plot of luminosity at 178 MHz against low frequency spectral index for the class C⁻ galaxies only. These two figures show that there is no relation between luminosity and either high or low frequency spectral index for the class C⁻ galaxies.

Fig. 1d, which is a plot of luminosity at 178 MHz against spectral index for the class S galaxies, shows a much greater correlation (coefficient = 0.66, significant at the 0.1% level). The regression relationship obtained, assuming spectral index x as the independent variable, is

$$\log L_{178} = 4.7x + 22.3 \quad (1)$$

It seems, therefore, that the correlation suggested by others was entirely a result of the presence of the class S sources, and that the class C⁻ sources only diluted the relationship.

It has been suggested⁵ that most of the unidentified sources in the 3CR catalogue are galaxies beyond the plate limit of the Palomar Sky Survey. Fifty-nine of the unidentified sources have class C⁻ spectra and twenty-one class S spectra. Assuming that these sources are indeed galaxies fainter than $m_v = 19.5$, we have indicated in Fig. 1b, c and d the regions where the majority of these sources must lie. The horizontal line is the lower bound to the luminosity determined for an unidentified source with a flux density of $9 \times 10^{-26} \text{ W m}^{-2} \text{ Hz}^{-1}$ and a distance given by the Hubble diagram for a galaxy with $m_v = 19.5$. The vertical arrows are drawn to include nearly all the sources. In Fig. 1b fifty-six of fifty-nine, in Fig. 1c fifty-five of fifty-nine, and in Fig. 1d nineteen of twenty-one of the unidentified sources are included within the arrows.

If, in the future, these sources are identified as galaxies, it is unlikely that a relationship between luminosity and spectral index will become visible for the class C⁻ galaxies. The range of spectral indices of the unidentified sources is similar to that of the galaxies (Fig. 1b and c). But the addition of unidentified sources to Fig. 1d might strengthen the relationship between luminosity and spectral index of class S sources because the range of spectral indices of the unidentified sources is limited to the range of the more luminous class S galaxies.

The monochromatic luminosities have been calculated at 178 MHz, because this is the frequency at which the sources were selected. But our conclusions are not dependent on the frequency at which the luminosities are calculated. The regression relationship for class S galaxies (equation (1)) may be generalized as follows:

$$\log L(f) = 4.7x + 22.3 - x \log \left\{ \frac{f_{\text{MHz}}}{178} \right\} \quad (2)$$

So calculating $L(f)$ at 1,400 MHz or 10,700 MHz yields similar regression relationships with reduced slopes of 3.8 and 2.9 respectively. In addition, a plot similar to Fig. 1b but based on luminosities calculated at 10.7 GHz shows no relationship between luminosity and spectral index for class C⁻ galaxies.

The existence of the luminosity-spectral index relationship for the class S galaxies requires that a spectral index-redshift relationship must also be present in the data. This follows because luminosity and redshift of 3CR galaxies are strongly correlated (distant sources must be very luminous to exceed the cutoff of the 3CR catalogue at $9 \times 10^{-26} \text{ W m}^{-2} \text{ Hz}^{-1}$). Data covering a greatly increased range of flux den-

sities will be required before it can be determined whether or not physical relationships, as distinct from mathematical correlations, exist among luminosity, spectral index, and redshift.

J. M. MACLEOD
L. H. DOHERTY

*Astrophysics Branch,
National Research Council of Canada,
Ottawa, Ontario*

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¹ Heeschen, D. S., *Pub. Astron. Soc. Pacific*, **72**, 368 (1960).

² Conway, R. G., Kellermann, K. I., and Long, R. J., *Mon. Not. Roy. Astron. Soc.*, **125**, 261 (1963).

³ Kellermann, K. I., Pauliny-Toth, I. I. K., and Williams, P. J. S., *Astrophys. J.*, **157**, 1 (1969).

⁴ Lynden-Bell, D., *Mon. Not. Roy. Astron. Soc.*, **155**, 119 (1971).

⁵ Bolton, J. G., *Astron. J.*, **74**, 131 (1969).

Photodetachment of Electrons from Major Negative Ions in the Lower D Region

THE relatively high atmospheric pressures in the lower D region of the ionosphere, at heights below about 80 km, permit the formation of negative ions by the three-body attachment of electrons to molecules or atoms. The photodetachment of electrons from negative ions by solar radiations has been used to explain increases in electron concentrations which occur prior to ground sunrise¹. Our theoretical study of the height distributions of electrons and negative ions shows the need to consider the detachment of electrons from the major negative ions throughout the day, to account for the daytime electron concentrations observed below 70 km.

Current ideas on the negative-ion chemistry of the D region are based on the rapid attachment of electrons to oxygen molecules to form O_2^- ions², and a sequence of reactions involving minor neutral constituents which leads to the formation of O^- , O_3^- , O_4^- , CO_3^- , CO_4^- , NO_2^- and NO_3^- ions³⁻⁶. It is probable that photodetachment from all these ions will occur in daytime but reliable laboratory measurements of photodetachment efficiencies are available only for O^- , O_2^- and O_3^- (ref. 7). We consider here the likely effects of including the photodetachment of electrons from other ions, such as CO_3^- , NO_2^- and NO_3^- .

Numerical solutions of the continuity equations for the eight negative-ion species listed above, and electrons, have been derived using laboratory measurements of rate coefficients²⁻⁶ and the height distributions of minor neutral constituents and ion-pair production rates shown in Fig. 1; the sum of positive-ion concentrations, considered as a single species, was found from the condition of charge neutrality. The O_2 distribution in Fig. 1 has been taken from the US Standard Atmosphere, 1962; that for CO_2 corresponds to a constant mixing ratio of 3×10^{-4} , equal to that in the lower atmosphere; those for O , O_3 and $\text{O}_2(^1\Delta_g)$ have been derived in a separate theoretical treatment; that for NO has been taken from the results of a rocket observation of the dayglow⁸ but has been extrapolated to heights below 70 km; and that for NO_2 has been taken from a theoretical study by Shimazaki and Laird⁹. The production rates correspond to the photoionization of NO and $\text{O}_2(^1\Delta_g)$, and the ionization of all atmospheric constituents by galactic cosmic rays; the values refer to a solar zenith angle of 45° and solar minimum conditions at middle latitudes. Allowance was also made in the calculations for the loss of electrons by the dissociative recombination of positive ions, assuming a rate coefficient of $10^{-5} \text{ cm}^3 \text{ s}^{-1}$ to be appropriate for water cluster ions, $\text{H}^+(\text{H}_2\text{O})_n$, known to be the dominant positive ions below 80 km¹⁰; small changes in this coefficient will not alter

the main conclusions of this study. The loss of negative ions by recombination with positive ions was also included, a rate coefficient of $10^{-7} \text{ cm}^3 \text{ s}^{-1}$ being adopted. This value is based on measurements of the recombination of negative ions with simple positive ions, such as NO^+ , and it is possible that larger values might be found for recombination with water cluster ions.

Fig. 2a represents the height distributions of negative ions and electrons deduced for day time steady-state conditions; photodetachment of electrons from only O^- , O_2^- and O_3^- ions is included using reported rates⁷. No measurements of negative-ion composition during daytime at middle latitudes are available for comparison. Reasonable agreement with measurements of electron concentrations¹¹ is found for heights above $\sim 70 \text{ km}$ but the rapid theoretical decrease with height shown at lower heights in Fig. 2a is not confirmed experimentally. For instance, concentrations near 60 km are typically greater than 10 cm^{-3} , some two orders of magnitude larger than the computed value. Fig. 2b shows the corresponding results obtained when allowance is made for the photodetachment of electrons from ions CO_3^- , NO_2^- and NO_3^- ; for NO_2^- a preliminary value of 0.04 s^{-1} is available⁷ for the photodetachment rate but for CO_3^- and NO_3^- estimated values¹² of 0.04 s^{-1} and 0.03 s^{-1} , respectively, have been adopted. It is seen that the electron concentrations below about 70 km are markedly enhanced relative to those of Fig. 2a, the values now being comparable with measurements. In addition the inclusion of photodetachment of electrons from CO_3^- , NO_2^- and NO_3^- has resulted in a change in the relative concentrations of electrons and negative ions, and of various negative-ion species. Further calculations have shown that the photodetachment of electrons from NO_3^- is chiefly responsible for the increased electron concentrations below about 70 km . Photodetachment of electrons from CO_3^- ions with a rate of 0.04 s^{-1} represents a relatively slow loss process for these ions compared with a reaction involving O atoms³. The concentrations of this ion and of other negative ions are increased as a result of the larger electron concentrations from photodetachment from NO_3^- . This release of electrons from NO_3^- is effective in maintaining reasonable electron concentrations at low heights even when the assumed photodetachment rate is reduced by an order of magnitude. It is evident that the minimum value of the photodetachment rate that will be effective will depend on the rate of loss of NO_3^- by recombination with water cluster ions. Results similar to those presented here could be produced by chemical detachment of electrons from NO_3^- but no such reaction has been reported to date.

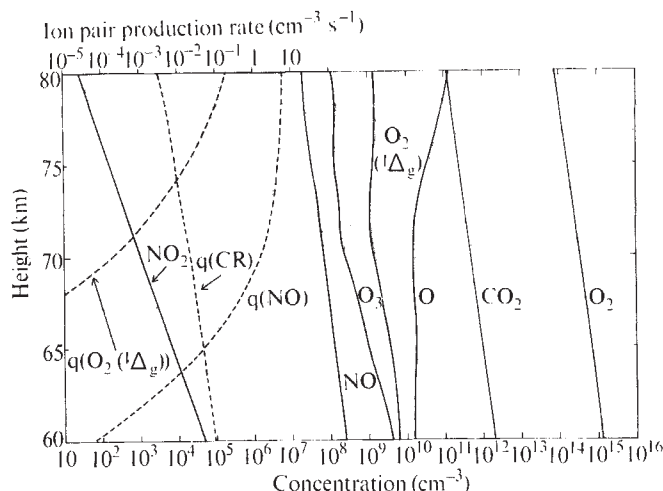


Fig. 1 Height distributions of neutral constituents and of ion pair production rates resulting from the ionization of NO by Lyman- α radiation ($q(\text{NO})$), of $\text{O}_2(^1\Delta_g)$ by $102.7\text{--}111.8 \text{ nm}$ radiation ($q(\text{O}_2(^1\Delta_g))$), and of all constituents by cosmic rays ($q(\text{CR})$).

Current ideas on negative-ion chemistry require processes for detaching electrons from NO_3^- ions in order to reproduce daytime measurements of electron concentrations below 70 km , and photodetachment could represent such a process. This result will not be affected significantly by any likely changes in the assumed distributions of neutral constituents. An added complication, however, could be the hydration of negative ions, as observed in the laboratory¹³. No quantitative study of hydration is possible at present and rocket-borne measurements of negative ions at high latitudes during nighttime^{14,15} have yielded conflicting evidence concerning the presence of hydrated ions.

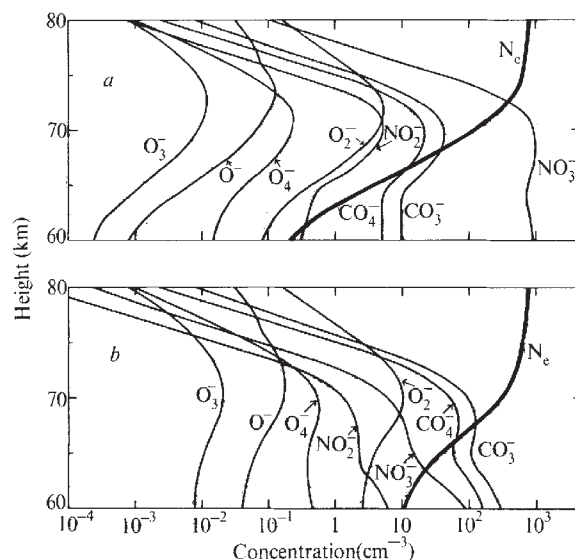


Fig. 2 Height distributions of the concentrations of electrons (N_e) and of different negative-ion species: a, without photodetachment of CO_3^- , NO_2^- and NO_3^- ; b, with photodetachment of CO_3^- , NO_2^- and NO_3^- .

A more complete description of these studies of the negative-ion composition of the normal D region and the changes associated with a solar flare is to be presented elsewhere.

L. THOMAS
P. M. GONDHALEKAR
M. R. BOWMAN

SRC Radio and Space Research Station,
Ditton Park,
Slough,
Buckinghamshire SL3 9JX

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- Moler, W. F., *J. Geophys. Res.*, **65**, 1459 (1960).
- Chanin, L. M., Phelps, A. V., and Biondi, M. A., *Phys. Rev. Lett.*, **2**, 344 (1959).
- Fehsenfeld, F. C., Schmeltekopf, A. L., Schiff, H. I., and Ferguson, E. E., *Planet. Space Sci.*, **15**, 373 (1967).
- Fehsenfeld, F. C., and Ferguson, E. E., *Planet. Space Sci.*, **16**, 701 (1968).
- Fehsenfeld, F. C., Albritton, D. L., Burt, J. A., and Schiff, H. I., *Canad. J. Chem.*, **47**, 1793 (1969).
- Fehsenfeld, F. C., Ferguson, E. E., and Bohme, D. K., *Planet. Space Sci.*, **17**, 1759 (1969).
- Defence Atomic Support Agency Handbook (1967), DASA Information and Analysis Center, General Electric, Tempo, Santa Barbara, California.
- Meira, L. G., *J. Geophys. Res.*, **76**, 202 (1971).
- Shimazaki, T., and Laird, A. R., *J. Geophys. Res.*, **75**, 3221 (1970).
- Narcisi, R. S., and Bailey, A. D., *J. Geophys. Res.*, **70**, 3687 (1965).
- Maeda, K., *J. Geomagn. Geoelect.*, **23**, 133 (1971).
- Kamiyama, H., *J. Geomagn. Geoelect.*, **22**, 403 (1970).
- Fite, W. L., and Rutherford, J. A., *Discuss. Faraday Soc.*, **37**, 192 (1964).
- Narcisi, R. S., Bailey, A. D., Della Luca, L., Sherman, C., and Thomas, D. M., *J. Atmos. Terr. Phys.*, **33**, 1147 (1971).
- Arnold, F., Kissel, J., Krankowsky, D., Wilder, H., and Zähringer, J., *J. Atmos. Terr. Phys.*, **33**, 1169 (1971).

A New Source for the Large-scale Electric Fields in the Magnetosphere

ALFVÉN¹ has pointed out that no real understanding of the properties of the magnetosphere is possible without a knowledge of the electric field. Large scale electric fields in the magnetosphere so far discussed in the literature have been confined mainly to the following three categories²: first, the electric field due to the polarization of the ambient plasma in the vicinity of the magnetopause through the frictional interaction with the solar wind; second, the electric field due to the polarization of energetic particles caused by differential drift motions of these charged particles in the Earth's magnetic field; and third, the penetration of the interplanetary electric field into the magnetosphere. But electric fields induced by variations of the magnetic field have not been reported until very recently. In 1971 Ferraro^{3,4} reported that inductive electric fields are unimportant during the growth of the ring current and Wang and Kim⁵ pointed out that the electric fields induced by a decaying ring current during the recovery phase of a magnetic storm are quite substantial and should not be ignored. Here we show, using model calculations, that a decaying ring current is capable of producing the large scale electric fields in the magnetosphere.

The existence of an extraterrestrial ring current encircling the Earth was postulated⁶⁻⁸ in the early 1930s in an attempt to account for the global depression of the horizontal component of the Earth's magnetic fields observed at low and middle latitudes during the principal phase of magnetic storms. Singer⁹ suggested that the ring current is a result of the guiding-centre motion of charged particles in the Earth's magnetic field. Experimental verification of such a large current system has been made only recently^{10,11}. The basic features of the ring current as revealed by the satellite observations were found to be consistent with the hydromagnetic theory of geomagnetic storms proposed by Dessler and Parker¹².

The storm-time ring current is generally enhanced during the period of the main phase of a magnetic storm by injections of energetic charged particles. The current intensity reaches its maximum at the maximum epoch of the magnetic storm and then subsides to its quiet-time intensity in the recovery phase, which generally takes 4 to 105 h^{13,14}.

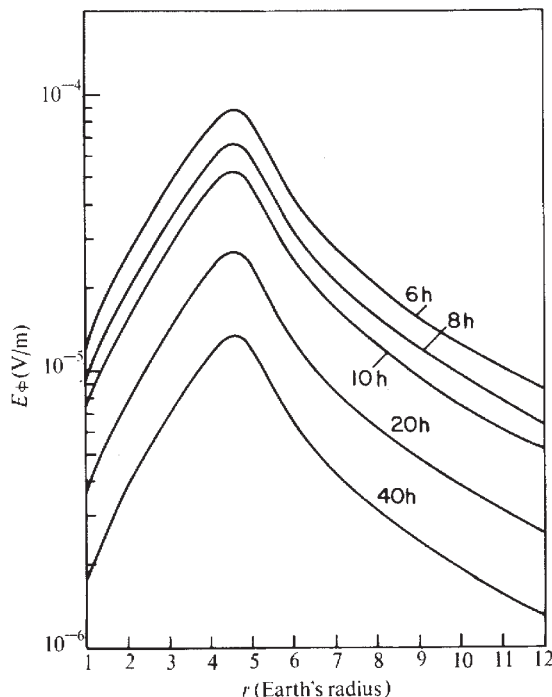


Fig. 1 Azimuthal electric fields induced by ring currents with decay time constants of 6, 8, 10, 20 and 40 h. $I = 3.7 \times 10^6$ A; $a = 4.6 R_E$; $\theta \sim 90^\circ$.

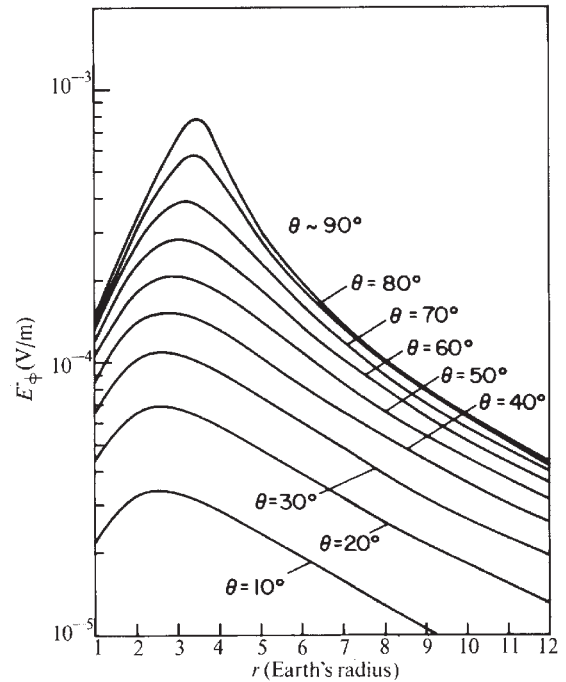


Fig. 2 Azimuthal electric fields for values of θ in the range $10^\circ \leq \theta \leq 90^\circ$. $I = 5.6 \times 10^6$ A; $\tau = 1$ h; $a = 3.5 R_E$.

For the calculation, we assume the ring current decays exponentially in the form of

$$I = I_0 e^{-t/\tau}$$

where I_0 is the current intensity at $t=0$ corresponding to the maximum epoch of the magnetic storm and τ is a decay time constant. The electric field induced by a decaying ring current for a system consisting of the Earth's dipole and the symmetric ring current is then

$$\mathbf{E} = \left(\frac{\mu_0 a}{\pi} \right) \left(\frac{I}{\tau} \right) (a^2 + r^2 + 2ar \sin \theta)^{-1/2} \left[\frac{(3-k^2)K_1 - 2E_1}{k^2} \right] \mathbf{e}_\phi$$

using spherical coordinates (r, θ, ϕ). The coordinate system is oriented so that the Earth's dipole is pointing in the positive z -direction and the ring current flowing eastward. For simplicity of calculation we use an idealized ring current of infinitesimal cross section at a distance a from the Earth's centre instead of one with finite cross section. K_1 and E_1 are complete elliptic integrals with argument

$$k = \left(\frac{4ar \sin \theta}{a^2 + r^2 + 2ar \sin \theta} \right)^{1/2}$$

The azimuthal electric fields induced at equator by ring currents with decay time constants of 6, 8, 10, 20 and 40 h are plotted against the geocentric distance in Fig. 1. The current intensity used in the calculation is 3.7×10^6 A, an amount capable of producing a magnetic field distortion on the Earth's surface of 33γ , corresponding to two-thirds of a 50γ magnetic storm. We estimate the ring current intensity from low-latitude magnetograms assuming two-thirds of the $D_{st}(H)$ decrease was due to the ring current. The maximum electric field generated is in the range of 0.01–0.1 mV/m (Fig. 1) in agreement with results obtained from the slow cross-L drifts of whistler ducts¹⁵. Because larger magnetic storms are not uncommon, it is likely that the magnitude of the induced electric field can be larger than the values given here. Moreover, there is evidence^{16,17} indicating that the ring current can have a shorter decay time constant, $\lesssim 1$ h. For a 100γ magnetic storm, a ring current located at $3.5 R_E$ (R_E is the radius of the Earth) with a decay time constant of 1 h can induce an electric field of 0.8 mV/m (Fig. 2), which is compatible with that

generated by the frictional interaction between magnetospheric plasma and the solar wind.

In our calculation, the ring current is assumed to be located at a fixed distance from the Earth. But observations have shown that a continuous slow shift of the location of the ring current is frequently the case. This shift of the position of the ring current will introduce an additional electric field; our preliminary calculations (unpublished) show that the electric field induced by the shift of the location of the ring current is less than 0.02 mV/m in the recovery phase of a magnetic storm.

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C. S. WANG
J. S. KIM

Department of Atmospheric Science,
State University of New York at Albany,
Albany, New York 12222

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- ¹ Alfvén, H., *Space Sci. Rev.*, **7**, 140 (1967).
- ² Obayashi, T., and Nishida, A., *Space Sci. Rev.*, **8**, 3 (1968).
- ³ Ferraro, V. C. A., *Geophys. J. Roy. Astron. Soc.*, **23**, 251 (1971).
- ⁴ Ferraro, V. C. A., *Geophys. J. Roy. Astron. Soc.*, **25**, 419 (1971).
- ⁵ Wang, C. S., and Kim, J. S., *IAGA Program and Abstracts*, 423 (Moscow, 1971).
- ⁶ Chapman, S., and Ferraro, V. C. A., *Terr. Magn. Atmos. Electricity*, **36**, 77 (1931).
- ⁷ Chapman, S., and Ferraro, V. C. A., *Terr. Magn. Atmos. Electricity*, **37**, 147 (1932).
- ⁸ Chapman, S., and Ferraro, V. C. A., *Terr. Magn. Atmos. Electricity*, **38**, 79 (1933).
- ⁹ Singer, S. F., *Trans. Amer. Geophys. Union*, **38**, 175 (1957).
- ¹⁰ Cahill, jun., L. J., *J. Geophys. Res.*, **71**, 4505 (1966).
- ¹¹ Frank, L. A., *J. Geophys. Res.*, **72**, 3753 (1967).
- ¹² Dessler, A. J., and Parker, E. N., *J. Geophys. Res.*, **64**, 2239 (1959).
- ¹³ Frank, L. A., *J. Geophys. Res.*, **75**, 1263 (1970).
- ¹⁴ Cahill, jun., L. J., *J. Geophys. Res.*, **75**, 3778 (1970).
- ¹⁵ Carpenter, D. L., and Stone, K., *Planet. Space Sci.*, **15**, 395 (1967).
- ¹⁶ Cummings, W. D., and Coleman, jun., P. J., *Radio Science*, **3**, 758 (1968).
- ¹⁷ Cornwall, J. M., Coroniti, F. V., and Thorne, R. M., *J. Geophys. Res.*, **76**, 4428 (1971).

Another Gondwanaland

THE grouping of the continents (South America, Africa, India, Australia and Antarctica) into a single continent (Gondwanaland) is now widely accepted for most of Palaeozoic and Mesozoic times, but there is still disagreement on the way in which the continents bordering the Indian Ocean fit into this picture. The most durable reconstruction has been that of du Toit¹, which was based on detailed geological information from many areas, and is similar to the geometric fit^{2,3}; this is generally consistent with palaeomagnetic data^{4,5}. It is, however, necessary to attribute the scatter of palaeomagnetic poles in Permo-Triassic times to significant multipole components, in addition to a predominantly axial geocentric dipole, when using this reconstruction⁶. I have pointed out⁷ that the positioning of Madagascar is critical in most postulated reconstructions of eastern Gondwanaland and that there is some evidence against the northerly derivation required for the Smith and Hallam² and du Toit¹ reconstructions.

Seafloor anomalies from most of the Indian Ocean⁸ do not show any large movement of Madagascar during the past 75 m.y., but the absence of significant reversals for the period immediately before the Tertiary sequence prevents unique interpretations for earlier times. Green⁹ has described parallel anomalies south-west of Madagascar which he interpreted as indicating that Madagascar has a southern origin, and moved eastwards from Natal before a northerly movement to its present location. The anomaly pattern found by Green is not a unique indicator of age, but suggests an origin in the Jurassic. Flores¹⁰ has also shown geological evidence for a Jurassic

derivation of the island from the Mozambique area; the Mozambique "bulge" consisting chiefly of Karroo lavas and later sedimentary deposits. This origin also seems to be consistent with models of the evolution from rift valley junctions (K. Burke and J. Whiteman, personal communication) with initial fracturing in the Jurassic (~160 m.y. BP) and later separation of the island from Africa. The biotic relationship between Madagascar and Africa¹¹ suggests fairly free migration between the two in Jurassic times, although increasing difficulty of migration is suggested until final separation was completed in the mid-Cretaceous. Recent work¹² shows that a marine connexion existed down the entire eastern coastline of Africa, suggesting that the initial fracturing was largely completed by the Jurassic, although the degree of separation of the continental landmasses is uncertain. This distribution of Jurassic seas, however, does invalidate the matching of Jurassic shorelines which was crucial in du Toit's arguments for the northerly positioning of Madagascar.



Fig. 1 Stereographic projection of another "Gondwanaland".

Because of the evidence for a southerly derivation of Madagascar, I have attempted a new geometric reconstruction (Fig. 1). This fit, which is only to a first approximation, places Madagascar against Mozambique (pole of rotation 57.7° S, 157.5° E, rotation by +11.0°); India close to rotated Madagascar (23.2° S, 156.3° W, +66.3°); Australia close to Antarctica (6.0° S, 40.5° E, -31.6°); and both continents against India-Africa (0.2° S, 43.4° W, +39.7°). In order to complete the "circumvallation" of Gondwanaland and to avoid overlap between Patagonia and the Antarctic Peninsula, it is necessary to unbend Patagonia slightly and to straighten the Antarctic Peninsula. These features are thought¹³ to have curled in the Tertiary and Cretaceous respectively. It is also necessary to move West Antarctic relative to East Antarctica¹⁴; this movement must have occurred in the Jurassic and was presumably related to the eruption of the Jurassic Ferrar and Tasmanian Dolerites. I believe that much of south-east Asia may also have formed part of Gondwanaland in the Palaeozoic and early Mesozoic, because there is biotic and geological evidence relating Burma, southern China and possibly southern Korea with Gondwanaland. The complex history of these regions, however, prevents a detailed reconstruction at present. Iran probably lay to the north of India and its northerly movement possibly formed the first significant land bridge between East Gondwanaland and Laurasia. The grazing connexion between north-west Africa and Laurasia was frequently impassable to terrestrial fauna because it was covered by shallow Tethyan seas for much of the time.

Table 1 The Scatter of Palaeomagnetic Poles

	All continents c.s.d.			All Gondwanan continents c.s.d.		
	N	New	S-H	N	New	S-H
Cambrian	4	36.8	36.5	3	39.7	39.2
Cambro-Ordovician	2	15.3	15.3	2	15.3	15.3
Ordovician	5	24.0	25.1	3	20.9	19.8
Silurian	2	19.8	18.6	1	—	—
Combined	13	27.1	26.8	9	28.6	27.6
Devonian	4	15.7	15.7	2	14.0	14.0
Carboniferous	6	23.9	21.5	4	26.1	22.6
Combined	10	32.0	31.1	6	32.5	30.4
Carbo-Permian	2	11.1	11.1	2	11.1	11.1
Permian	3	17.3	19.5	1	—	—
Permo-Triassic	1	—	—	1	—	—
Triassic	6	10.9	12.8	4	9.0	12.4
Combined	12	17.2	17.1	8	16.6	15.8
Jurassic	4	14.3	13.1	2	5.4	8.4
Jurassic-Cretaceous	1	—	—	1	—	—
Cretaceous	6	18.0	19.0	4	14.7	15.7
Combined	11	15.7	15.6	7	11.9	12.5

N is the number of continental average poles used; South America-Africa and Australia-Antarctica are combined because they have identical relationships on both reconstructions. The circular standard deviation (c.s.d.) is given for the new and the Smith and Hallam (S-H) reconstructions using Laurasian and Gondwanan data averaged for each geological period.

I tested the geometric fit against the most reliable palaeomagnetic data available^{1,5,7} (Table 1). The test depends on the assumption that the most realistic fit will result in the least scatter of the average geomagnetic poles for the continents for each geological period prior to the separation of the different continental fragments. In general, the scatter of poles is marginally greater during early Palaeozoic times on the new reconstruction, but the differences are mostly less than 1° on fairly large scatters, while for periods when the data are more numerous and more reliable, the scatter of poles is consistently reduced on the new reconstruction. I believe this to be significant.

There is therefore good geological and geophysical evidence in support of a southerly derivation of Madagascar; this defines more closely a reconstruction which appears to be more consistent with the palaeomagnetic evidence. It seems therefore to be premature to attribute the palaeomagnetic scatter observed using the Smith and Hallam reconstruction to non-axial geocentric components of past geomagnetic fields.

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D. H. TARLING

Department of Geophysics and Planetary Physics,
University of Newcastle upon Tyne

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- ¹ du Toit, A. L., *Our Wandering Continents* (Oliver and Boyd, Edinburgh and London, 1937).
- ² Smith, A. G., and Hallam, A., *Nature*, **225**, 139 (1970).
- ³ Dietz, R. S., and Holden, J. C., *J. Geophys. Res.*, **75**, 4939 (1970).
- ⁴ Creer, K. M., *Nature*, **204**, 369 (1964).
- ⁵ Creer, K. M., *Nature*, **207**, 51 (1965).
- ⁶ Briden, J. C., Smith, A. G., and Sallomy, J. T., *Geophys. J.*, **23**, 101 (1971).
- ⁷ Tarling, D. H., *Nature*, **229**, 17 (1971).
- ⁸ McKenzie, D. P., and Sclater, J. G., *Geophys. J.*, **24**, 437 (1971).
- ⁹ Green, A. G., *Nature Physical Science*, **236**, 19 (1972).
- ¹⁰ Flores, G., *Trans. Geol. Soc. S. Africa*, **73**, 1 (1970).
- ¹¹ Keast, A., in *Evolution, Mammals and Southern Continents* (Univ. New York Press, 1972).
- ¹² Dingle, R. V., and Klinger, H. C., *Nature Physical Science*, **232**, 37 (1971).
- ¹³ Dalziel, I. W. D., Kligfield, R., Lowrie, W., and Opdyke, N. D., *Eos*, **52**, 921 (1971).
- ¹⁴ Dalziel, I. W. D., and Elliot, D. H., *Nature*, **233**, 246 (1971).
- ¹⁵ Creer, K. M., *Earth Sci. Rev.*, **6**, 369 (1970); Tarling, D. H., *Principles and Applications of Palaeomagnetism* (Chapman and Hall, 1971).

A Year of Strain Measurements in Southern California

STRAIN measurements have been obtained nearly continuously for over a year from a surface laser interferometric strainmeter¹ deployed at Piñon Flat Geophysical Observatory. This site (33.49° N, 116.46° W) is located in the wedge of granite that forms the San Jacinto Mountains, between the San Andreas fault and an active branch, the San Jacinto fault, to the south of their confluence. The area lies at the southern end of the trace of the 1857 Fort Tejon earthquake (magnitude 8.5) and was possibly loaded with 10 m of potential displacement by that event. To the south, in the Imperial Valley, a roughly continuous fault movement occurs accompanied by moderate seismicity, whereas to the north no recent motion has been detected along some 350 km of the San Andreas fault which is aseismic at present.

The strainmeter is situated 15 km from the San Jacinto fault and 25 km from the San Andreas fault, oriented north-south at about 45° to the trend of the fault system. It is basically a Michelson interferometer, measuring the changes in distance between two piers 800 m apart. The decomposed granite which covers the surface at the site grades into granitic bedrock 3 to 10 m below and forms the matrix into which these piers are set. Strain is measured in terms of the laser wavelength by fringe counting, so the instrument is digital. As a result, it has a large dynamic range and requires no calibration as well as being linear and wideband (DC to 1 MHz). The observatory now contains north-south and east-west strainmeters, two-component tiltmeters, a La Coste tidal gravimeter, and a microbarograph. The observations reported here were obtained on the north-south strainmeter.

Data from the instruments are sampled at one second intervals and recorded, with the time, on magnetic tape. Some on-line data reduction is performed which allows a slower data logging system to sample once every 6 min without aliasing. This system has digital printout and provides the data for tidal and secular studies. The output from each instrument is also recorded on a strip chart.

Fig. 1 shows a section of the raw strain data with a low-pass filtered version superimposed. Over this one month period, the average strain rate was approximately 8.4×10^{-9} day⁻¹ in the sense of ground expansion. Fig. 2 shows the low-pass filtered record for the entire year. During this period, the net strain accumulated was about 1.5×10^{-7} and the maximum accumulated strain was 8×10^{-7} . It seems from Figs. 1 and 2 that grossly misleading estimates of the yearly secular strain would be obtained by extrapolation from several months' data. For example, extrapolation from the data of Fig. 1 predicts a year's accumulated strain of 3.1×10^{-6} ; the actual value is a factor of 20 smaller.

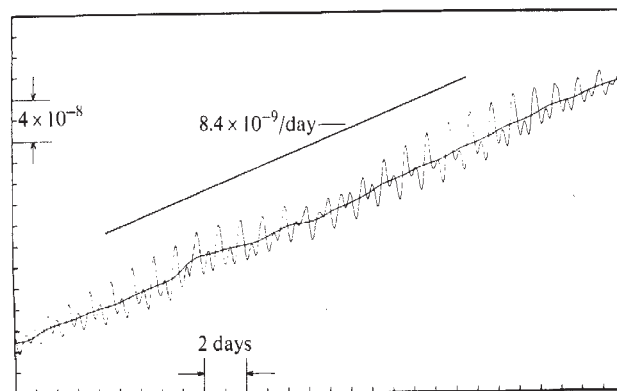


Fig. 1 Strain data from day 153 to day 182 (June 2 to July 1, 1971) with a filtered trace superimposed. The filter used removes frequencies above 1/40 cycle per h and introduces no phase shift.

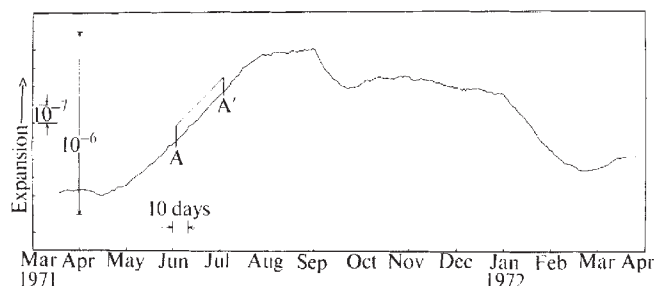


Fig. 2 A year of secular strain data. All frequencies above 1/40 cycle per h have been removed. The raw data from section A-A' are shown in an expanded form in Fig. 1.

The chief sources of error in the accumulated strain are: first, extrapolation error; second, pier tilt; and third, laser frequency instability. These are discussed below. First, during the period of observation the instrument was operational roughly 90% of the time with the longest period of downtime being 11 days. Most of the downtime (particularly the longer periods) resulted from mechanical problems associated with a new installation and these have since been remedied. The remaining downtime arose from local power failures, but these were short lived and do not adversely affect the measure of secular strain rates because the character of the record did not change significantly during them, and the Earth tides may be used to extrapolate across the gaps. We estimate that the accumulated error from downtime extrapolation over the year is less than 5×10^{-8} strain.

Second, differential tilt of the instrument piers introduces a noise into the strain record at approximately 2×10^{-9} strain per μ rad of differential pier tilt. By monitoring the laser beam position at the far end we estimate the maximum strain from interferometer pier tilt as less than 10^{-7} yr $^{-1}$. Tilt was not measured on the remote pier during this period; we assume that its behaviour was similar to that of the instrument pier.

Third, because the distance between end piers is measured in terms of the laser wavelength, there is no way of distinguishing changes in strain from changes in laser wavelength. Because of the Doppler broadening of the atomic spectral lines, lasing action can occur over a wavelength range of ~ 0.002 nm. The spectral linewidth therefore determines the laser's output wavelength to $\sim 3 \times 10^{-6}$ ($\Delta\lambda/\lambda$). The exact output wavelength is determined principally by the laser cavity dimensions. To achieve more stability, the laser used in this instrument is wavelength controlled by reference to a quartz Fabry-Perot etalon. Noise originating from this source is caused by pressure and temperature fluctuation of the etalon as well as by long term creep in the quartz. We

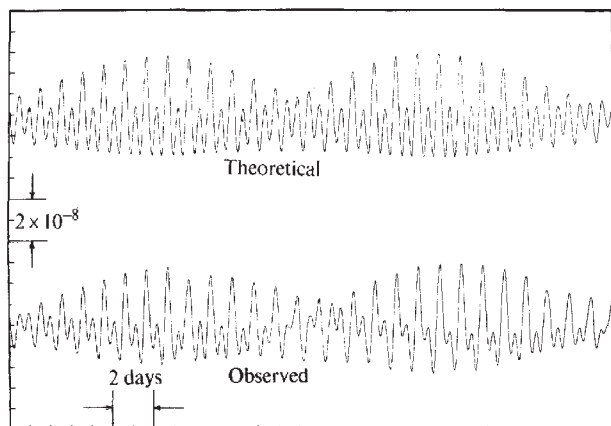


Fig. 3 Strain record, during the same period shown in Fig. 1, has been filtered to remove all frequencies below 1/40 cycle per h. The theoretical strain tides, calculated for an elastic oceanless Earth, are shown for comparison.

believe that the noise arising from the pressure and temperature variations is small ($< 10^{-9}$ strain yr $^{-1}$) but the record may be contaminated by an unknown amount of noise resulting from creep of the quartz. We do not think it likely that this contamination is large compared to the other noise, but experiments with two etalons are now being carried out as a check.

Fig. 3 shows the part of the record removed by the low pass filter used in Fig. 2. The theoretical tides, calculated for an elastic oceanless Earth, are plotted on the figure for comparison. The differences between observation and theory can be explained in part by the loading of the crust by the ocean tides. Each line in the spectrum of Earth tides has a corresponding peak in the ocean height spectrum. These peaks, however, are not in phase with the Earth tide peaks, so both the amplitude and phase of the ocean tides must be known before corrections can be made. For the semidiurnal tide along the coast of California a good model exists² which allows this calculation to be made with some confidence. The observed tide agrees with the tidal model in this case, to within the experimental errors, and the goodness of fit indicates the accuracy of the overall instrument-site calibration. Unfortunately, the offshore tides are not well known at other frequencies so that this procedure has been successful only when applied to the semidiurnal tides.

In summary, the year's observations of Earth strain at this site indicate a net strain accumulation of 1.5×10^{-7} in the sense of north-south expansion. The errors we attach to this figure are of the same order of magnitude as the accumulated strain and are somewhat subjective. Further observations, however, will reduce the errors by reducing extrapolation errors, revealing any differential pier tilt and providing a measure of relative frequency drift in the north-south and east-west strainmeters.

Our measurement of accumulated strain is immediately significant, however, because it sets an upper limit on the secular strain rate for the year which is smaller than that reported before and an order of magnitude smaller than the figure usually quoted for California³. The general character of the record tempts us to predict an annual strain cycle at this site of $\sim 4 \times 10^{-7}$ amplitude.

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JON BERGER
FRANK WYATT
RALPH LOVBERG

*Institute of Geophysics and Planetary Physics,
University of California, San Diego,
La Jolla, California*

Received April 3, 1972.

¹ Berger, J., and Lovberg, R. H., *Science*, **170**, 296 (1970).

² Munk, W., Snodgrass, F., and Wimbush, M., *Geophys. Fluid Dynamics*, **1**, 161 (1970).

³ Benioff, H., *Bull. Geol. Soc. Amer.*, **70**, 1019 (1959).

BIOLOGICAL SCIENCES

Circadian Variation of RNA in Human Leucocytes

DIURNAL changes in RNA content have been described for animal cell lines such as liver¹ and epithelium², and so it would seem logical that similar periodic changes in RNA would occur *in vivo* in other eukaryotic cells.

In humans, the leucocyte is a readily accessible cell. A diurnal variation in the number of circulating leucocytes in man has previously been reported³, but periodic RNA changes in human leucocytes. This report shows changes in total (quantitative) RNA content, as well as changes in RNA type (qualitative) in human leucocytes, following a rhythm of approximately 24 hours, that is, circadian. Utilizing advances in electroencephalographic investigation, diurnal evaluation has been extended to include samples during normal sleep.

Five healthy young adults were studied (4 males and 1 female), with an age range from 20 to 25 years (mean age 23.8 years). Three subjects had blood drawn at 0600, 1200, 2200 and 0200 hours, while the other two had blood drawn at 1200, 2200, 0200, 0600, 1200 and 2200 hours. All subjects had their night-time sleep recorded by means of electrodes attached to the scalp, as described in the standard manner for EEG recording, and a continuous all-night EEG recording was made. This ensured that the amount and type of sleep obtained by the subjects during this period were not different from the normal EEG sleep pattern for the subject's age and sex, as determined by previous work^{4,5}.

Fifty ml. of heparinized venous blood was drawn and mixed with cold 3% 'Dextran' (m.w. 170,000) in a 2 'Dextran' to 1 blood ratio, mixed with ten inversions and left standing at room temperature for half an hour. The resulting supernatant contained a 75% yield of leucocytes⁶. A 0.1 ml. aliquot of this suspension was counted in a 'Coulter' counter, and RNA was extracted using a modification of the technique described by Cooper⁷.

The suspension was centrifuged at 48,000g for 30 min at 4° C. The leucocyte pellet was resuspended and thoroughly mixed in 3 ml. of buffer (acetate buffer, 0.01 M, pH 5.1, with 0.1 M NaCl and 0.001 M MgCl₂ with 1% bentonite at 0° C), and the solution was then frozen at 4° C overnight.

The solution was then thawed and blended in a hand micro-blender for 3 min. The resultant suspension was centrifuged at -4° C at: 3,020g for 10 min; 10,800g for 15 min; and 34,800g for 15 min. The solution was decanted and the supernatant diluted with equal parts of 0.15 NaCl and 0.001 M EDTA containing 2.0 mg/ml. bentonite. The suspension was then made 0.5% sodium dodecyl sulphate and shaken for 20 s on ice. Three ml. of redistilled buffer saturated phenol containing 0.1% 8-OH-quinoline was added, and the mixture was shaken gently for 5 min at 65° C.

The mixture was then chilled rapidly and the emulsion broken by centrifugation at 0-4° C (1,000g) for 10 min. The aqueous phase was made 0.2 M with potassium acetate and two volumes of alcohol were added. After freezing for 18 h at -20° C, the precipitate was recovered by centrifugation. The

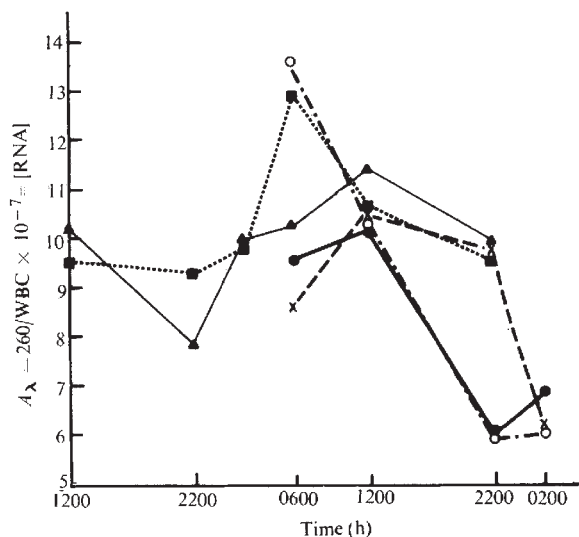


Fig. 1 Circadian changes in total human leucocyte RNA. ●—●, 1; ×—×, 2; ○—○, 3; ▲—▲, 4; ■—■, 5.

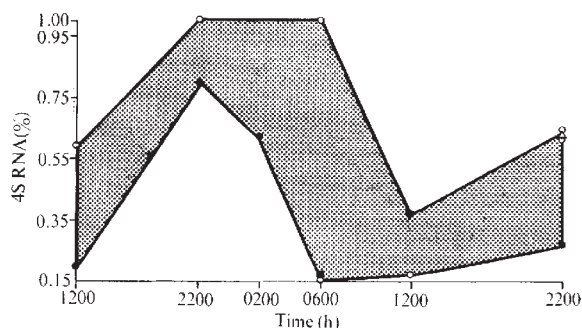


Fig. 2 Circadian changes in 4S human leucocyte RNA.

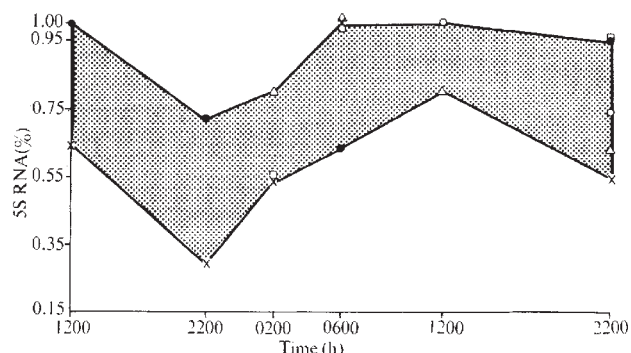


Fig. 3 Circadian changes in 5S human leucocyte RNA.

pellet was then washed twice with 70% ethanol, and dissolved in 0.5 ml. of the original buffer. Gel electrophoresis was carried out using the discontinuous electrophoresis technique of Ornstein⁸, employing a 5% polyacrylamide gel.

RNA components were identified by staining with toluidine blue and scanning in a 'Gilford' spectrophotometer with a polygraph write-out.

The type of RNA was ascertained by the relative mobility of peaks as compared with known standard tRNA and sRNA from *Escherichia coli* and *Tortula* (Sigma). Relative proportions of 4S and 5S RNA were compared using a compensating planimeter.

Total supernatant RNA (total RNA) was obtained by measuring absorbance ('Gilford' spectrophotometer) at 260 nm and 280 nm.

A definite circadian rhythm in total RNA is present in all five subjects (Fig. 1). Relative total RNA per cell was obtained by dividing the absorbance at 260 nm by the number of cells. The peak RNA is found between 2200 and 0200 hours, whereas the low point is at about 0600 hours. Figs. 2 and 3 depict circadian changes in 4S and 5S RNA.

Individual 4S and 5S peaks were compared. The largest area was designated as 100% and the other areas expressed as a percentage thereof for constructing the figures. The highest values of 4S RNA occurred between 2200 and 0200 hours, whereas the highest value for 5S RNA occurred between 0600 and 1200 hours. This also held true if the absolute values were plotted. It is further noted that the 4S curve has a significant inverse relationship to the 5S, and the curve of the latter is similar to the curve for total RNA.

As seen in Table 1, the minimum value of cells per ml. of supernatant occurred at 1600 hours. Comparing this with Fig. 1, showing changes in total RNA content, we see that the low point for total number of cells corresponds to the high point for total amount of RNA per cell.

Periodic variations in the metabolic processes of cells have previously been documented^{1,2}. A higher content of nucleic acid has been found in the morning as compared with evening in epithelium of cornea, tongue, liver and thymic lymphocytes of rats and mice. The circadian fluctuation of RNA in the human leucocyte is described here for the first time; our finding that a

Table 1 White Cells per Cubic Millimetre of Supernatant

Subject	1200	2200	0200	Hours	0600	1200	2200	0200
1	—	—	—	2100	2100	2023	3624	3345
2	—	—	—	2000	2256	3170	3276	—
3	—	—	—	2100	2383	3525	3568	—
4	2533	3031	2767	2430	2617	3066	—	—
5	2895	2875	2773	2102	2560	3019	—	—

low point of circadian change of total white cells occurs at 0600 hours (Table 1) agrees with the other work³.

The qualitative changes in 4S and 5S RNA and the significance of their inverse change remain to be further elucidated. An analysis of higher molecular weight RNA is currently being performed in our laboratory using an agarose polyacrylamide system.

Recent technological advances in the electroencephalographic investigations of human sleep now provide a means for relating the consequences of different stages of sleep to cyclic variation of RNA. Implications for a possible anabolic role of sleep need to be considered. In view of the effect of chemotherapeutic agents on nucleic acid and protein biosynthetic pathways, a circadian variation in RNA also has implications for drug therapy in infection and leukaemia.

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WILLIAM C. KOHLER
ISMET KARACAN
OWEN M. RENNERT

Departments of Paediatrics, Biochemistry and Psychiatry,
University of Florida College of Medicine,
Gainesville, Florida 32601

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¹ Barnum, C. P., Jaretzky, C. D., and Halberg, F., *Amer. J. Physiol.*, **195**, 301 (1958).

² Alov, I. A., *Bull. Exp. Biol. Med.*, **48**, 1418 (1959).

³ Sharp, G. W. G., *J. Endocrinol.*, **21**, 107 (1960).

⁴ Williams, R. L., Agnew, H. W., and Webb, W. B., *Electroenceph. Clin. Neurophysiol.*, **17**, 376 (1964).

⁵ Williams, R. L., Agnew, H. W., and Webb, W. B., *Electroenceph. Clin. Neurophysiol.*, **20**, 264 (1966).

⁶ Skoog, W. A., and Beck, W. S., *Blood*, **11**, 436 (1956).

⁷ Cooper, H. L., *J. Biol. Chem.*, **243**, 34 (1968).

⁸ Ornstein, L., *Ann. NY Acad. Sci.*, **121**, 321 (1964).

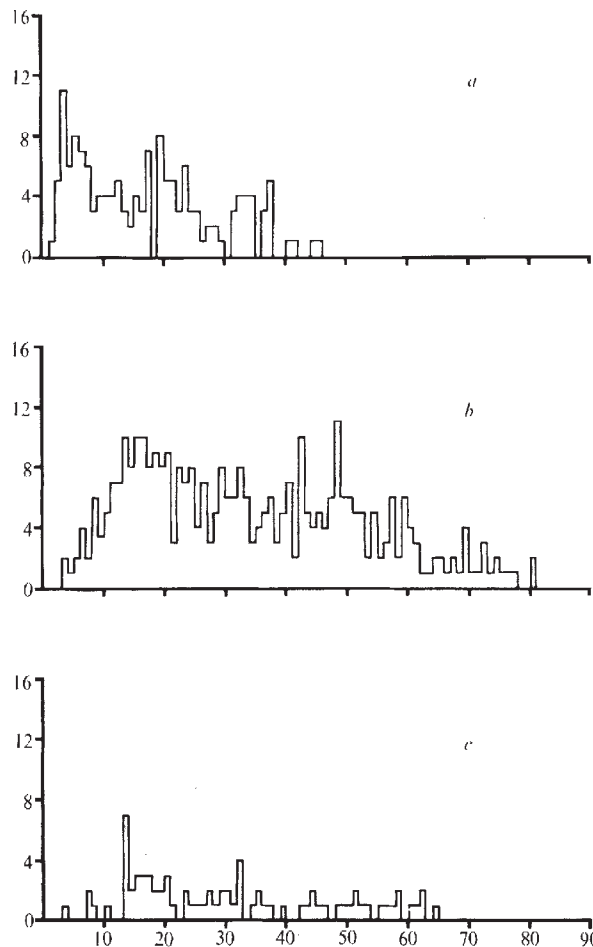


Fig. 1 Typical histograms of twitch frequency of individual mice as a function of time elapsed after intraperitoneal injection of *a*, 20 mg/kg compound IIb.HCl; *b*, 40 mg/kg mescaline HCl; and *c*, 160 mg/kg compound IIa.HCl. The total number of twitches, integrated over the observation period, are *a*, 149; *b*, 353; *c*, 77.

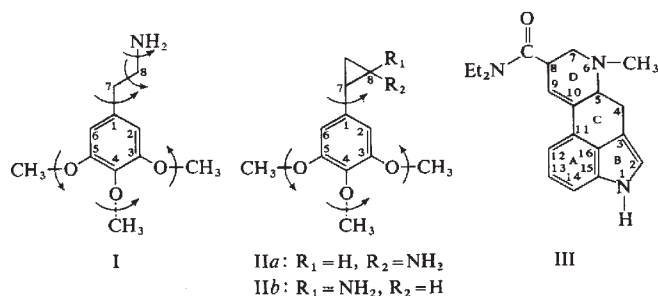
examine possible stereochemical preferences of the receptor for mescaline and other hallucinogenic phenethylamines.

Mescaline possesses a number of rotational degrees of freedom as depicted by the arrows on its structural formula. If not for energy barriers which inhibit free rotation, the mescaline molecule can assume an infinite number of different conformations. The magnitude and location of existent energy barriers will vary to some extent according to the physical environment, and positive drug-receptor interactions might impose additional restrictions on the conformation of mescaline and related compounds.

Possible alternative orientations of the methoxyl groups in mescaline have been considered in detail by Chothia and Pauling¹. These authors as well as others^{2,3} have made proposals concerning the conformation of the ethylamine side chain, which can rotate about the C₁-C₇, C₇-C₈ and C₈-N bonds, and the relative orientation of the aromatic ring with respect to bond C₇-C₈. The proponents have selected among four principal alternatives which are the two in-plane conformations (C₇-C₈ coplanar with the aromatic ring) with C₁-C₇ and C₈-N either *cis* (α) or *trans* (β), and the two out-of-plane conformations (C₇-C₈ normal to the aromatic ring) with C₁-C₇ and C₈-N either *cis* (γ) or *trans* (δ). The various choices were made on the basis of their structural congruity with one or the other portion of the lysergic acid diethylamide (LSD) molecule (III), or with regard to energetic favourability or crystallographic data. Although Neville *et al.*⁴ and Bailey⁵ have provided nuclear magnetic resonance (NMR) data in support of *trans* conformations for some related phenethylamine-type drugs, there has been no direct pharmacological

Stereochemical Requirements of the Mescaline Receptor

This report describes some psychopharmacological properties of two conformationally restricted analogues of the psychotomimetic compound mescaline (I). The geometrical distinction between these *cis* (IIa) and *trans* (IIb) isomers of 2-(3,4,5-trimethoxyphenyl)-cyclopropylamine makes it possible to



comparison of receptor preferences among the proposed conformations for mescaline and its congeners.

We prepared⁶ compound IIb, which approximates *transoid* conformers β or δ , and found that, in rodents, it has behavioural effects resembling those due to mescaline. Subsequently⁷, we synthesized compound IIa, which closely resembles *cisoid* conformers (either α or γ) of mescaline, with the objective of resolving part of the stereochemical controversy by pharmacological means.

The behavioural effects of compounds IIa (20 mg/kg), IIb (20 mg/kg) and of mescaline (40 mg/kg) were compared using mice and following our earlier procedure⁶. We have found that the syndrome produced by mescaline, and consisting principally of hypoactivity associated with crouching and characteristic twitching of the hindquarters is reproducible, dose-dependent and relatively specific for phenethylamine-type hallucinogens. Fig. 1 shows the manually-recorded time distribution of twitch frequency for three of the animals treated with the respective subject compounds. Compound IIb is qualitatively indistinguishable from mescaline in this test (Fig. 1a and b respectively) although its potency is somewhat greater and its duration of action briefer than that of mescaline. By contrast, compound IIa is almost inactive in producing the mescaline syndrome in terms of twitch frequency at comparable dosages. When the dosage is increased eight-fold (160 mg/kg) over that of its geometric isomer IIb, a mescaline-like response is obtained (Fig. 1c).

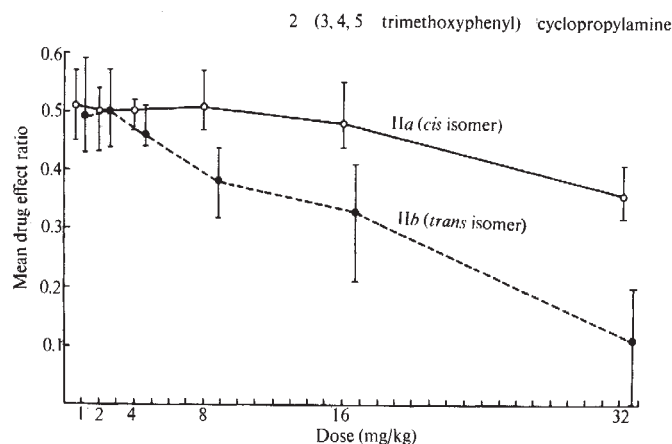


Fig. 2 Means and ranges of DRL-LH schedule performance for compounds IIa.HCl and IIb.HCl over six dose levels. See text for an explanation of the drug effect ratio.

The relative potencies of the two isomers were also examined in rats which had been trained on a schedule of differential reinforcement of low rate with a limited hold contingency (DRL15-LH5). This schedule has been useful for the assessment of psychotropic drugs such as amphetamine⁸, mescaline⁹ and tetrahydrocannabinol¹⁰. The results of primary interest are shown in Fig. 2, in which the "drug effect ratio" refers to schedule performance efficiency of the animals in the drug-treated and in the untreated condition¹⁰. Since a value of less than 0.5 for this ratio indicates decreased efficiency as a result of drug administration, Fig. 2 shows that compound IIa reduces efficiency to a significant extent only at the 32 mg/kg dosage level whereas its geometric isomer IIb produces a significant decrease in efficiency at the 8 mg/kg level.

Fig. 3 shows randomly selected daily records of the performance of two animals given 32 mg/kg of either IIa or IIb compared with their pre-drug day and post-drug day performance. These records indicate that whereas the overall response rates are little affected by drug treatment, compound IIa, and more particularly IIb, decreases the rate of reinforced responding, as has already been reflected by the data of Fig. 2. Although the temporal-spaced response deficit was evident at all effective dose levels, very few of these records showed the prolonged

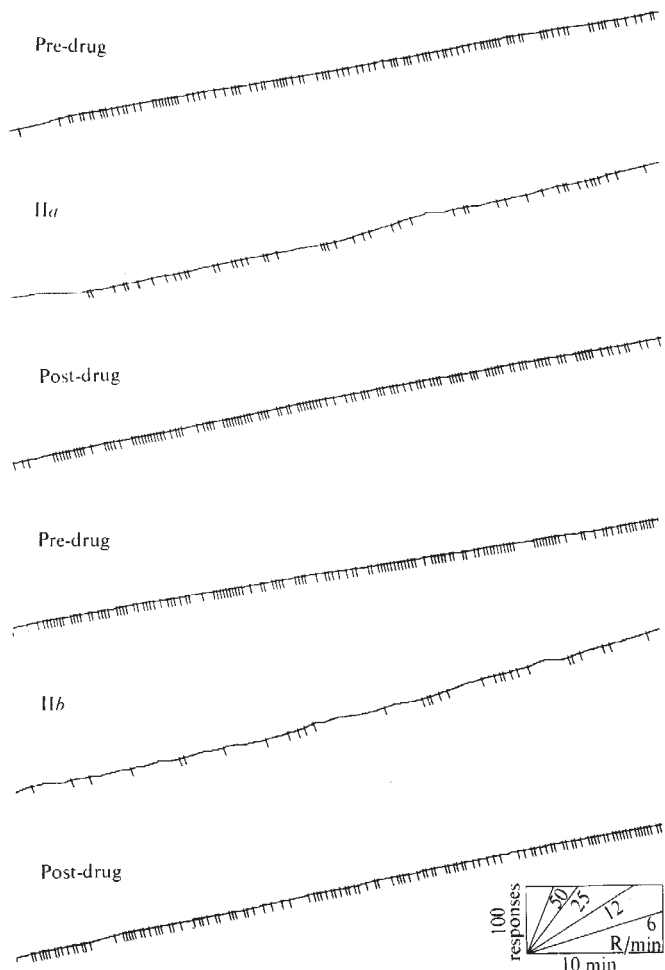


Fig. 3 Cumulative records obtained from rats treated with compound IIa.HCl (32 mg/kg) or IIb.HCl (32 mg/kg). Slope of line is a direct function of rate of responding. Reinforcements are indicated by downward pips. Records obtained on the day before drug (pre-drug) and on the day after drug (post-drug) are shown, respectively, above and below the drug day records.

periods of cessation of responding previously reported due to mescaline^{9,10}. But visual examination of the rats during the sessions, and independent observation of these sessions presented in random order on videotape, showed that rats treated with compound IIb (16 and 32 mg/kg) exhibited mescaline-like changes in gross motor behaviour resembling those seen in mice, whereas rats which had received compound IIa could not be distinguished from the saline controls.

The pronounced differences in the psychopharmacological properties of compounds IIa and IIb are of dual interest. First, it has been shown that such quantitative differences are absent in the parent, nuclear-unsubstituted *cis* and *trans*-2-phenylcyclopropylamines in their activity as monoamine oxidase inhibitors either *in vitro*¹¹ or *in vivo*^{12,13}. The introduction of nuclear methoxyl substituents, as in the pair, IIa and IIb, is therefore responsible not only for qualitative distinctions between the pharmacology of *trans*-2-phenylcyclopropylamine and IIb⁴, but also for quantitative differences between the potency ratios of these closely related pairs of geometric isomers. Second, if we assume that the more intense mescaline-like action of isomer IIb is consequent to its superior effect at the site of action (rather than to its resistance to metabolic degradation, an alternative possibility), we are encouraged to support the concept of a *transoid* conformation for mescaline at the receptor level.

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P. D. COOPER

Section of Biochemical Pharmacology,
Faculty of Pharmacy,
University of Montreal

G. C. WALTERS

Department of Psychology,
University of Toronto

Received December 23, 1971; revised February 14, 1972.

- ¹ Chothia, C., and Pauling, P., *Proc. US Nat. Acad. Sci.*, **63**, 1063 (1969).
- ² Snyder, S. H., and Richelson, E., *Proc. US Nat. Acad. Sci.*, **60**, 206 (1968).
- ³ Kang, S., and Green, J. P., *Proc. US Nat. Acad. Sci.*, **67**, 62 (1970).
- ⁴ Neville, G. A., Deslauriers, R., Blackburn, B. J., and Smith, I. C. P., *J. Med. Chem.*, **14**, 717 (1971).
- ⁵ Bailey, K., *J. Pharm. Sci.*, **60**, 1232 (1971).
- ⁶ Walters, G. C., and Cooper, P. D., *Nature*, **218**, 298 (1968).
- ⁷ Cooper, P. D., *Canad. J. Chem.*, **48**, 3882 (1970).
- ⁸ Zimmerman, J., and Schuster, C. R., *J. Exp. Anal. Behav.*, **5**, 497 (1962).
- ⁹ Smythies, J. R., Johnston, V. S., and Bradley, R. J., *Brit. J. Psychiat.*, **115**, 55 (1969).
- ¹⁰ Webster, C. D., Walters, G. C., and Willinsky, M. D., *Nature* (in the press).
- ¹¹ Sarkar, S., Banerjee, R., Ise, M. S., and Zeller, E. A., *Helv. Chim. Acta*, **43**, 439 (1960).
- ¹² Costa, E., *The Pharmacologist*, **1**, 82 (1969).
- ¹³ Tedeschi, D. H., Tedeschi, R. E., and Fellows, E. J., *Proc. Soc. Exp. Biol. Med.*, **103**, 680 (1960).

Keratoblast and Keratocyte, not Keratinocyte

CELLS of both the reproductive and functional compartments of oral and cutaneous epithelia are at present indiscriminately called "keratinocytes". The two types of cells, however, differ markedly in function and fate, and it is therefore logical, as well as useful, to distinguish them by different terms. It is therefore proposed to introduce the term "keratoblast" to denote a cell in the reproductive compartment of these tissues, and thus to distinguish dividing cells from those that produce keratin.

The principal cellular components of oral and cutaneous epithelia fall conveniently into two compartments, namely a proliferative or reproductive compartment and a functional or mature compartment. The cells of the proliferative compartment, found chiefly in the basal cell layer, are reproductive and end by mitotic division. The cells of the functional compartment, found chiefly in the spinous, granular and cornified cells layers, produce keratin and later die and desquamate. The proliferative compartment, having retained its reproductive capacity, constitutes the germinative population for the functional compartment.

The difference between these two types of cells is important enough to warrant a separate name for each, and there are precedents for a semantic distinction between mature and immature cells—for example, chondroblast and chondrocyte, fibroblast and fibrocyte, osteoblast and osteocyte.

"Keratoblast" is the logical name for a reproductive cell of oral and cutaneous epithelia. It connotes "progenitor of keratocyte" and is preferred to "keratinoblast", which is not a well formed word. The Greek word from which these words are derived is "keratos", meaning "horn", and the stem is "kerato-", not "keratino-".¹

The term "keratinocyte", unfortunately, is not accurate. The proper form, as explained in the preceding paragraph, is "keratocyte". There is an analogy here with the closely related melanin-producing cells which are correctly termed "melanocytes" rather than "melaninocytes". It should be mentioned

also that "keratinize" and its derivatives, "keratinized" and "keratinization" are not correct, the proper forms being "keratize", "keratized" and "keratization" respectively.

Neither "keratinocyte" nor the other words improperly formed with "keratino-" have yet been included in either *Nomina Anatomica* or *Nomina Histologica*; therefore it is to be hoped that "keratocyte" and the other words properly formed with "kerato-" will be generally accepted as the preferred terms.

A. IAN HAMILTON

Department of Restorative Dentistry,
School of Dentistry,
University of Washington,
Seattle, Washington 98195

Received February 14; revised March 2, 1972.

- ¹ Roberts, F., *Medical Terms*, fourth ed. (Heinemann, London, 1966).

Effect of PABA on Chloroquine Resistance in *Plasmodium berghei* yoelii

IN mice¹ paraaminobenzoic acid (PABA) in the diet of the host is necessary for the growth of blood infections of *Plasmodium berghei*. The area of the parasite metabolism in which PABA is involved is believed to be closely associated with the point of action of certain antimalarial drugs including pyrimethamine, proguanil and the sulphonamides. Several drugs of this group are antagonized by PABA². The effect of PABA on the antimalarial action of chloroquine has received little attention as chloroquine does not seem to act directly on the pathways of PABA metabolism. Kirakosyan³, however, has shown that chloroquine is more effective against a sensitive strain of *P. berghei berghei* in mice when PABA is absent from the diet of the host.

I have examined the effect of different dietary concentrations of PABA on the degree of chloroquine resistance of strains of *P. berghei yoelii*. All strains of *P. b. yoelii* show resistance to the drug⁴, but results from different laboratories on the degree of resistance are sometimes inconsistent. The present results provide a probable explanation for these discrepancies.

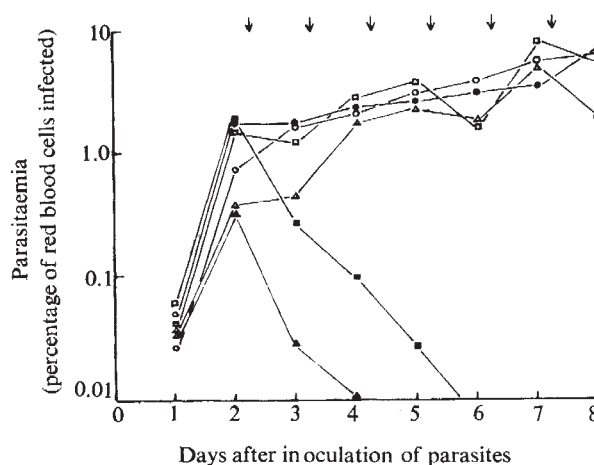


Fig. 1 The effect of different dietary concentrations of PABA on the response to chloroquine of blood infections of *P. b. yoelii* (strain 33X). Each point represents the average parasitaemia of five mice. ●, 5×10^{-3} g PABA per 100 ml. in drinking water; ■, 2.5×10^{-3} g PABA per 100 ml. in drinking water; ▲, 1.25×10^{-3} g PABA per 100 ml. in drinking water; ○, 40 mg chloroquine per kg mouse weight administered; ○, distilled water administered; ↓, time at which chloroquine or distilled water was administered.

Two strains of *P.b. yoelii*, 33X and 5CP, were studied. Groups of ten mice were placed under a series of different PABA regimes in their drinking water 48 h before the inoculation of parasites. The mice were fed on a solid diet low in PABA. Each mouse was inoculated with 10^6 parasitized blood cells; 48 h after the inoculation of parasites chloroquine was injected intraperitoneally in 0.1 ml. of distilled water into five mice of each group. The control series received an equivalent inoculation of distilled water alone. This treatment was repeated at 24 h intervals for 6 days, parasitaemia counts being made approximately 18 h after each inoculation.

The results for strain 33X are presented in Fig. 1. At the greatest concentration of PABA the parasites are totally resistant to the chloroquine. At a lesser concentration of PABA, however, the infection is rapidly eliminated by chloroquine treatment, although in the absence of chloroquine the parasitaemia rises at a rate scarcely less than that of the animals receiving maximum PABA. Similar results were obtained for strain 5CP.

These results demonstrate that chloroquine resistance in *P.b. yoelii* is dependent on the concentration of PABA in the diet of the host.

The emergence of chloroquine resistance in falciparum malaria in South East Asia and South America represents a major setback to malaria therapy⁵. Should this resistance be similar to that found in *P.b. yoelii*, a study of the effect of PABA on the chloroquine response of resistant strains of *P. falciparum* could aid the solution of this problem.

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RICHARD CARTER

*Protozoan Genetics Unit,
Institute of Animal Genetics,
West Mains Road,
Edinburgh EH9 3JN*

Received January 24; revised February 24, 1972.

¹ Jacobs, R. L., *Exp. Parasitol.*, **15**, 213 (1964).

² Aviado, M. D., *Exp. Parasitol.*, **25**, 399 (1969).

³ Kirakosyan, S. D., *Medskaya Parazit.*, **39**, 662 (1970).

⁴ Warhurst, D. C., and Killick-Kendrick, R., *Nature*, **213**, 1048 (1967).

⁵ *Resistance of Malaria Parasites to Drugs, Tech. Rep. Ser.*, World Health Org., 296 (1965).

Evidence for the Existence of Latent Myxoma Virus in Rabbits (*Oryctolagus cuniculus* (L.))

STUDIES carried out at Urana, New South Wales, from 1950 to 1953¹ showed that the pattern of myxomatosis epidemiology in Australia consists of regular, summer mosquito-borne epizootics, in which most susceptible animals are infected and a large percentage die. Recent studies (unpublished results of R. T. W. and I. P.) indicate a much reduced case mortality rate but a similar epidemiology.

These epizootics last for only a few months, and so the means by which the myxoma virus remains in a population between epizootics is important for the long term survival of the virus. Fenner and Ratcliffe¹ concluded that sufficient animals escaped infection each summer for the disease to trickle over the winter period transmitted by vectors of low efficiency. Indeed, it has been stated² that attenuated viruses, which allow a diseased rabbit to remain alive for longer than those infected with a virulent strain, would be preferentially selected for, thus assisting the virus to overwinter.

A possible alternative method of overwintering is by means of "carrier" animals, which had recovered from myxomatosis, but still harbour the virus in a latent, symptomless form. In

certain circumstances the virus might be reactivated, multiply, and become transmissible. Such a possibility has usually been discounted³.

This latter possibility was suspected in two outbreaks of myxomatosis which occurred in the winter of 1969 and which were studied in detail^{4,5}. These suspicions were strengthened in December 1969 following an epizootic in an enclosed rabbit population at Canberra. This population consisted of adult rabbits which had been captured alive in September 1969 and their progeny, born during October–November 1969. On December 17, 1969, there were sixty-one adults and their 120 offspring. Blood samples of the sixty-one adults were tested by Sobey *et al.*^{6,7} for the presence of soluble antigens of myxoma, or their antibodies; forty-two showed antibodies, and the other nineteen no antibodies.

On December 17, 1969, eleven cases of myxomatosis were noted in the enclosure, and the ensuing epizootic lasted until late January 1970. Of the eleven, seven were adult animals which had previously shown circulating antibodies to myxoma virus. Of the other four cases, two were susceptible adults and two were young animals born in the enclosure. Thus, some animals thought to be immune were not, although all reports (with one exception which has not been repeated⁸) have led to the assumption that recovery from myxomatosis confers lifelong immunity to a second challenge of myxoma virus². Also, if the virus had been introduced from outside by a flying vector, it must have selectively transmitted the virus to the immune animals, as they were outnumbered by susceptibles 42:139 at that time. The probability of this distribution of the disease among the three categories of rabbits is very remote ($P < 0.001$).

It seems more likely that some of the "immune" animals in the Canberra enclosure were carrying latent myxoma virus, which was reactivated early in December 1969 and transmitted to the susceptible rabbits. Accepting this, there were several conditions prevailing at the time which could have caused it. These included hormonal stresses (the rabbits having just ceased breeding), thermal stress (consequent on the onset of summer) and social stresses (due to overcrowding).

The next experiment was an attempt to cause reactivation of latent virus in "immune" rabbits by exposing them to "thermal" and "hormonal" stresses.

On March 10, 1970, twenty of the surviving "immune" rabbits from the previous enclosure experiment were placed in individual cages in a temperature controlled room. During the next 7 months they were subjected to two periods of heat stress and one of cold stress (Fig. 1). During the first period of heat stress, half the rabbits were injected daily with 10 IU of ACTH and the other half with 2.5 IU.

Only one rabbit (No. 13) showed generalized signs of myxomatosis, and this was during the first period of heat stress. She was an animal which originally had a very low level of circulating antibody but which had shown no signs of myxomatosis during the December/January epizootic. On May 13, 10 days after being exposed to the heat stress, she had very swollen eyelids, and 10 days later she showed the signs of an advanced case of myxomatosis, including swollen conjunctiva, a discharge from the eyes, together with the involvement of the ano-genital region. Soluble myxoma antigens were found in the blood of rabbit No. 13 in three of the six blood samples taken on May 15 and 21. She continued to show signs of the disease for 6 weeks, after which she recovered (Fig. 2). On May 6, a swab of the eye discharge from rabbit No. 13 was taken and injected into each of two susceptible domestic rabbits. Both showed typical signs of myxomatosis, and both died, one after 13 days and the other after 16 days.

It is considered that this recovery of myxoma virus from one of twenty healthy "immune" rabbits in a room which permitted no entry of virus provides convincing evidence for the existence of latent myxoma in certain *O. cuniculus*.

Several other animals developed a reddening and swelling of the lids of only one eye (Fig. 3), and a purulent discharge from

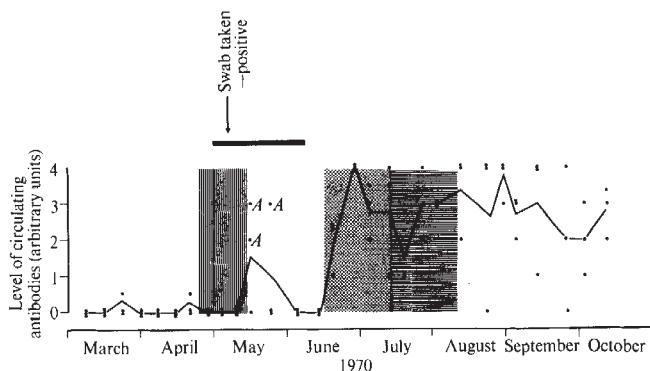


Fig. 1 The level of the circulating antibody of myxomatosis of rabbit No. 13 from March 10 until October 13, 1970. Rabbit No. 13 was one of 20 rabbits immune to myxomatosis which survived a previous enclosure experiment. She had an antibody level of 1 when placed in the enclosures on September 19, 1969. The twenty rabbits were all healthy when placed into individual cages in a temperature controlled room on March 10, 1970. The temperature was maintained at 15.6° C (60° F), except where indicated in the following experiments. Three blood samples were obtained from each rabbit weekly for serological examination, and the rabbits examined. *a*, From April 22 until May 11 (indicated by the vertical line shading in the figure), the temperature was increased to 23.9° C (75° F), and no vegetables were supplied. In addition, ten of the animals received a daily injection of 2.5 IU of ACTH, and the other ten 10 IU of ACTH. Rabbit No. 13 received 10 IU of ACTH. *b*, From June 18 until July 10 (stippled shading in figure), the temperature was decreased to 1.7° C (35° F), and again no green vegetables were provided. *c*, From July 10 until August (horizontal shading in figure) the temperature was increased to 26.7° C (80° F). On May 1, rabbit No. 13 first showed signs of myxomatosis. On May 31, she was recovering (see Fig. 2) (solid horizontal line in the figure). A sample of the eye discharge obtained on May 6 initiated myxomatosis in two susceptible domestic rabbits, which died 13 and 16 days after challenge (arrowed in figure). Also, three of the six blood samples taken during the third week of May showed the presence of circulating myxoma antigen (*A* in figure).

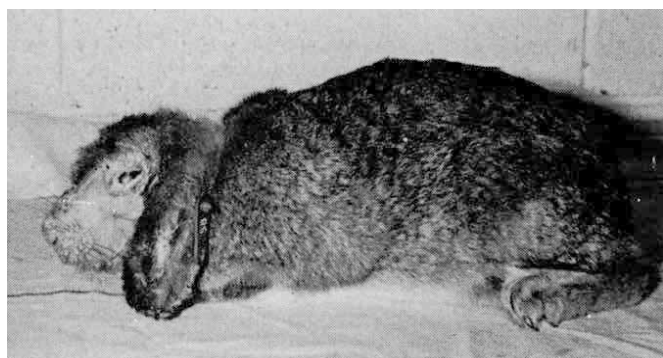


Fig. 2 Rabbit No. 13 taken on May 31, showing clear signs of myxomatosis 39 days after heat and hormonal stress.

that eye, during the two periods of "heat" stress. None of this discharge initiated myxomatosis in susceptible domestic rabbits.

The preceding evidence suffers in that the primary challenge of the "immune" rabbits by myxoma virus was never observed but inferred from the serological evidence of circulating antibodies in the blood of the animals. Partly to correct this deficiency, and to gain information on the seasonal levels of antibody to myxoma virus in rabbits that have recovered from the disease, litters from immune does were obtained from September to December 1970. The kittens were challenged with grade III (field) strain of myxoma virus at either 3 or 10 weeks of age. All survived, and when recovered they were removed to a rabbit-proof enclosure, and examined at frequent intervals. They bred in spring 1971, and by July 22 there were twenty-three young susceptible rabbits in the enclosure.

From August 10 until September 9, an epizootic of myxomatosis spread through the young rabbits. On September 9, an adult rabbit (No. 128) which had been challenged on

December 14, 1970, appeared to be suffering from myxomatosis, and a sample of his eye discharge initiated myxomatosis in two susceptible domestic rabbits, which died after 20 and 23 days.

When initially challenged in December 1970, rabbit No. 128 had displayed the typical signs of myxomatosis, and he had recovered 5 weeks later. His blood samples showed the presence of circulating antibodies to myxoma virus after this challenge (Fig. 4).

Thus, a rabbit which has been observed to recover from myxomatosis, and then appear healthy, is able to shed myxoma virus at a later date. In this case, the second appearance of

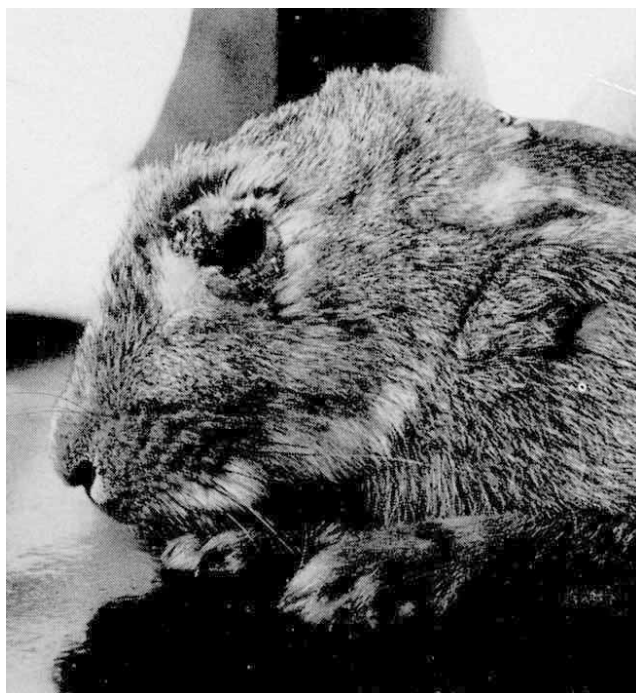


Fig. 3 One of the six rabbits which showed a swelling of, and discharge from, one eye only during periods of heat stress. The other eye remained perfectly healthy.

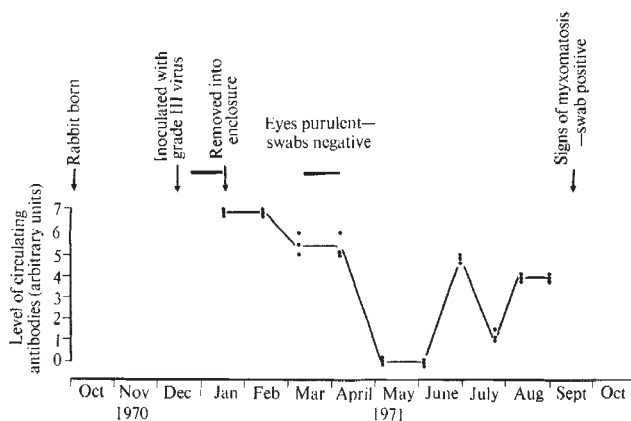


Fig. 4 The level of circulating antibody to myxomatosis of rabbit No. 128 during 1971. Rabbit No. 128, a male, was one of seventeen rabbits injected at 10 weeks of age with a grade III (field) strain of myxoma virus (arrowed in figure). All the rabbits survived the challenge, and were released into a half-acre rabbit-proof enclosure when they recovered (arrowed in figure). These rabbits produced litters in March and April 1971, during which time (solid bar in figure) rabbit No. 128 was producing a purulent discharge from both eyes. This discharge failed to initiate myxomatosis in susceptible domestic rabbits. Myxomatosis spread through the young animals from August 10 until September 9, 1970. On September 9, rabbit No. 128 appeared to have signs of myxomatosis. A sample of the eye discharge initiated myxomatosis in two susceptible domestic rabbits, which died after 20 and 23 days (arrowed). Three weeks later, on October 5, rabbit No. 128 had no visible signs of myxomatosis.

virus could have been due to re-infection, but we consider, for several reasons, that reactivation of latent virus occurred.

These observations demonstrate the possibility that: a rabbit which has recovered from myxomatosis is able to shed myxoma virus at a later date; latent myxoma virus may be reactivated in rabbits under various forms of stress, and may be transmitted from them; epizootics of myxomatosis may be initiated from rabbits in which latent myxoma virus is reactivated.

These results have been obtained with rabbits from three widely differing regions of New South Wales (Mogo, on the coast; Urana, a Mediterranean-type climate; Canberra on the slopes of the Great Dividing Range). It seems likely that many, if not most, epizootics in Australia are initiated by rabbits in which latent myxoma virus has been reactivated. Only three epizootics of myxomatosis have been studied in sufficient detail in recent years (refs. 4 and 5 and unpublished work of R. T. W., P. J. Fullagar, C. Kogon, and C. C. Davey), and in each case virus reactivation was considered to be a likely source of the virus which initiated them.

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R. T. WILLIAMS
J. D. DUNSMORE
I. PARER

Division of Wildlife Research,
CSIRO,
PO Box 84,
Lyneham, Canberra ACT 2602

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¹ Fenner, F., and Ratcliffe, F. N., *Myxomatosis*, 379 (Cambridge University Press, 1965).

² Fenner, F., *Rec. Austral. Acad. Sci.*, **1**, 131 (1967).

³ Vaughan, H. E. N., and Vaughan, J. A., *Symp. Zool. Soc.*, London, **24**, 289 (1968).

⁴ Dunsmore, J. D., Williams, R. T., and Price, W. J., *Austral. J. Zool.*, **19**, 275 (1971).

⁵ Williams, R. T., Fullagar, P. J., Davey, C. C., and Kogon, C., *J. Appl. Ecol.* (in the press).

⁶ Sobey, W. R., Conolly, D., and Adams, K. M., *Austral. J. Sci.*, **28**, 354 (1966).

⁷ Sobey, W. R., Conolly, D., and Adams, K., *Austral. J. Sci.*, **32** (8), 331 (1970).

⁸ Fenner, F., and Marshall, I. D., *J. Hyg.*, Cambridge, **52**, 321 (1954).

Incidence of Epidermal Lesions in Fish of the North-East Irish Sea Area, 1971

THE fish of the North-East Irish Sea (Fig. 1) and Solway Firth area have been surveyed continuously by ourselves and others since 1959 at least. During the period until the end of 1970, epithelial lesions were not observed to a significant degree. In March 1971, however, concurrent with observations made by fishermen, a marked incidence of such disease was noted in plaice (*Pleuronectes platessa*) and dab (*Limanda limanda*) (Fig. 2). A confirmed occurrence off the coasts of Cumberland, Kirkcudbrightshire, as well as unconfirmed reports from Luce Bay, Wigtownshire, and Bahama Bank, Isle of Man, indicate that fish from some 1,500 square miles of sea were affected.

The lesions belonged to three principal categories: lymphocystis, epidermal ulcers and fin damage. Lymphocystis (Fig. 3) has been noted periodically in the Irish Sea area before 1971¹. The epidermal ulcers did not have the typical appearance of bacterial ulcers in fish skin (Fig. 4), and the fin damage, characteristically of the lateral and caudal fins, produced erosion or total loss of these organs (Fig. 5).

With the exception of a single juvenile plaice, which had fin damage, the symptoms of each disease were confined to plaice and dab which were longer than 170 mm and the disease was most evident in fish more than 250 mm long. At such a size plaice have spent at least one winter in deep water and enter

Table 1 Numbers of Fish Taken during 1971, Ranked in Order of Abundance

Species	Beam	Otter/Seine	Total
<i>Pleuronectes platessa</i> (plaice)	276	8,545	8,821
<i>Limanda limanda</i> (dab)	322	2,279	2,601
<i>Buglossidium luteum</i> (solenette)	375	1	376
<i>Trisopterus luscus</i> (bib)	120	1	121
<i>Merlangius merlangus</i> (whiting)	106	1	107
<i>Pomatoschistus minutus</i> (sand goby)	101	—	101
<i>Raja clavata</i> (roker)	3	56	59
<i>Gadus morhua</i> (cod)	19	36	55
<i>Platichthys flesus</i> (flounder)	3	32	35
<i>Callionymus reticulatus</i> (dragonet)	31	—	31
<i>Agonus cataphractus</i> (pogge)	27	—	27
<i>Solea solea</i> (dover sole)	17	2	19

A further nineteen species, *Trachinus vipera*, *Lophius piscatorius*, *Raja naevus*, *Aspitrigla cuculus*, *Scophthalmus rhombus*, *Microstomus kitt*, *Callionymus lyra*, *Rhinonemus cimbrius*, *Liparis liparis*, *Scophthalmus maximus*, *Scylliorhinus caniculus*, *Pomatoschistus microps*, *Sprattus sprattus*, *Sygnathus typhle*, *Scomber scombrus*, *Gadus minutus*, *Clupea harengus*, *Liparis montagu*, and *Gymnammodytes semisquamatus*, which occurred in small numbers brought the total of fish examined to 12,436.

Fig. 1 The North-East Irish Sea area. (1) Workington Bank, (2) English Channel, (3) Saltom Bay, (4) N St Bees Head, (5) S St Bees Head, (6) Sellafield, (7) Ravenglass, (8) Drigg, (9) Eskmeals, (10) Bahama Bank, (11) Little Ross.

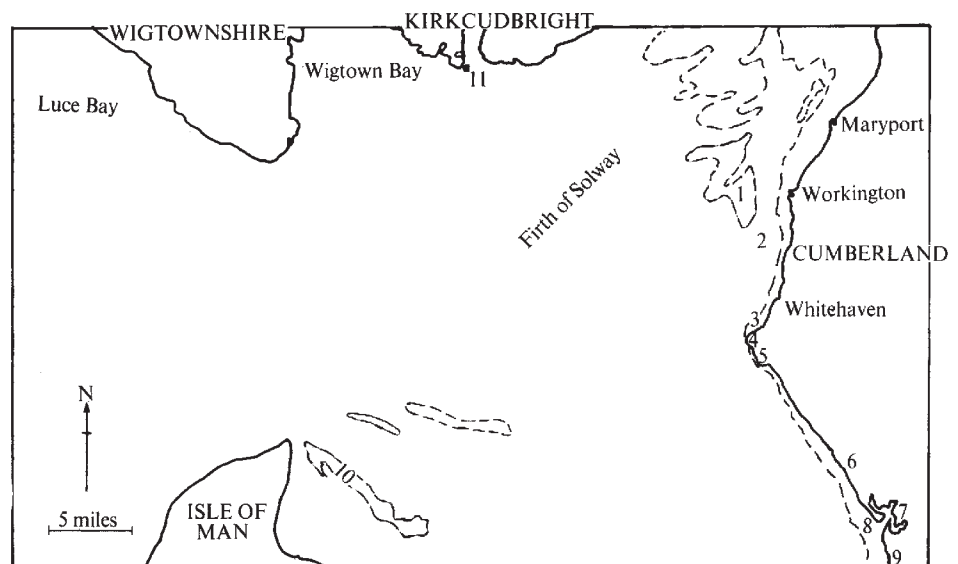


Fig. 2 Incidence of disease in pleuronectids of different sizes. *A*, Total plaice population excluding that of, *B*, Ravensglass-Eskmeals population of mid-September; *C*, total dab population. □, Number of fish; ■, number of diseased fish.

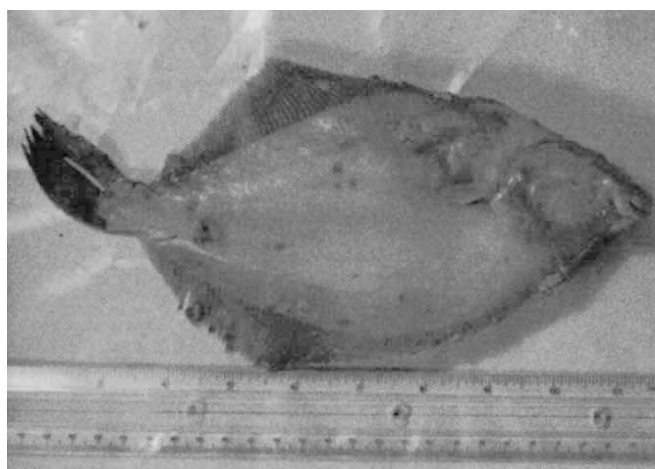
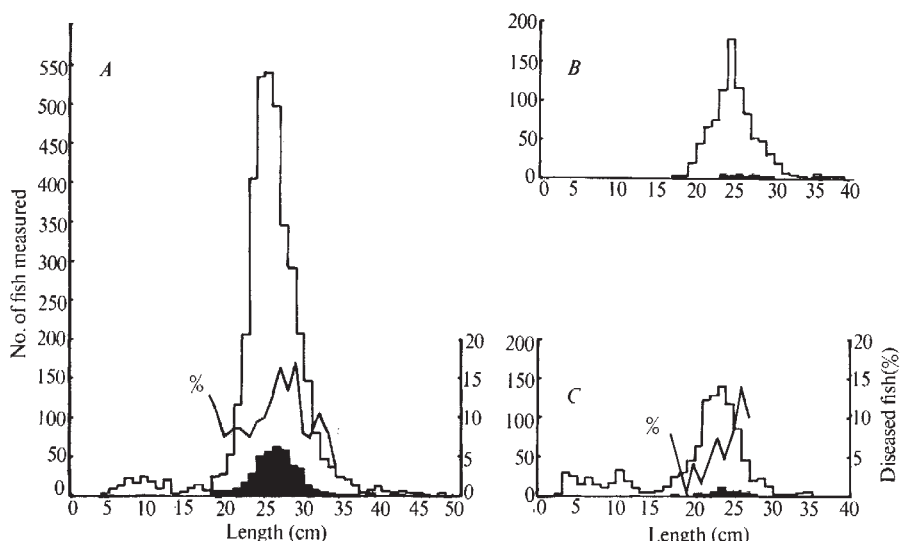


Fig. 3 Lymphocystis disease of plaice.

the fishery at 250 mm long. Dab enter the fishery at 150 mm long, and at 250 mm they are significantly older than plaice of the same length and have spent several winters in deep water.

Few multiple infections were noted in the individual fish. The mean incidence of these diseases in plaice ranged from 5.7%–6.2% in May/June and September respectively; in dab, on the other hand, the mean incidence fell from 9.1% in May/June to 4.1% in September. The fishing pressure on dab in this area is low: they are not favoured by fishermen, who return them promptly to the sea. On the other hand, only undersized and diseased plaice are returned to the sea; this return of diseased fish associated with a high fishing effort produced highly variable results in the incidence of diseased fish (up to 38%).

Table 2 Incidence of Epidermal Lesions in Fish larger than 150 mm Length, 1971

	Mean percentage incidence of disease.					
	<i>P. platessa</i>		<i>L. limanda</i>		<i>P. flesus</i>	<i>T. luscus</i>
	May–June	September	May–June	September		
Lymphocystis	1.6	4.6	1.6	1.5	12.5	—
Ulcers	1.9	1.3	4.0	1.7	0	A single adult with ulcer taken
Fin damage	2.3	1.1	1.9	1.2	0	—
Miscellaneous	—	2.2	—	0.3	0	—
Grand mean	5.7	8.4	9.1	4.4	12.5	—
	7.1		6.5			
MII	1.10	1.08	1.02	1.02	1.00	—

It seems likely, however, that the basal incidence of diseased plaice may have been of the order of 2.4%.

Epidermal neoplasms and ulcers have been reported from pleuronectids and soles of the US Pacific Coast. Here the incidence ranged from 0.7% to 6.4% and was not confined to the more adult fish^{2–4}. Lymphocystis outbreaks of up to 50% incidence have been reported from the North Sea⁵, and repeated epidemics of this disease have been reported in the pleuronectids of North-West European waters^{1,5}.

It must be recognized that epidemics occur periodically, but for three diseases to enter such a phase simultaneously and occur in plaice taken from some 1,500 square miles of sea suggests an abnormal situation. The Irish Sea has been used for dumping latterly, however, and was the scene of the sea-bird wreck in

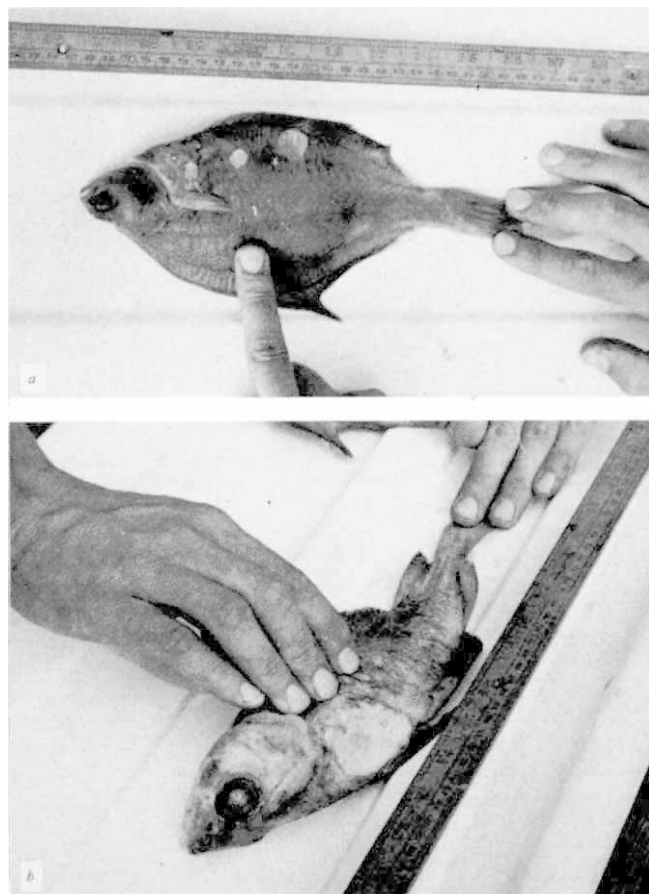


Fig. 4 *A*, Ulcer of dab. *B*, Ulcer of bib.

Table 3 Incidence of Individual Diseases in the Plaice and Dab of the North-East Irish Sea Area, 1971

Species	Area	N	Lymphocystis			Incidence of disease (%)					
			Mean	Maximum	Minimum	Mean	Maximum	Minimum	Fin damage		
<i>Pleuronectes platessa</i> (plaice)	Workington Bank	3	1.3	2.3	0.7	1.1	2.8	0	1.7	3.9	0.5
	English Channel	6	3.2	8.9	0	4.1	11.8	0.4	2.7	6.1	0.2
	Saltom Bay—	4	1.3	2.7	0	1.6	2.6	0	1.1	3.1	0
	N St Bees Head	*8	5.9	15.8	0	2.1	5.3	0	1.4	3.2	0
	S St Bees Head—	3	1.0	2.5	0	1.5	2.4	1.1	3.0	4.4	1.1
	Sellafield	*4	5.2	17.6	0	1.5	2.4	1.1	2.3	4.4	0
	Sellafield—Drigg	3	4.6	10.5	1.0	1.1	2.0	0.3	1.3	1.9	0.6
	Ravenglass—Eskmeals	2	0.8	1.5	0.2	0.5	0.7	0.2	0.7	1.1	0.3
	Little Ross	3	3.0	4.3	2.1	1.5	1.8	1.0	1.8	3.6	0.5
<i>Limanda limanda</i> (dab)	Workington Bank	3	0	—	—	0	—	—	0	—	—
	English Channel	4	7.8	25.7	0	2.8	10.8	0	2.1	5.1	0
	Saltom Bay—	4	0.3	1.0	0	5.3	10.7	1.7	3.7	10.7	0
	N St Bees Head	*8	1.7	6.3	0	4.6	10.7	1.3	3.5	10.7	0
	S St Bees Head—	3	0	—	—	2.3	6.0	0	0.2	0.7	0
	Sellafield	*4	1.0	3.9	0	2.3	6.0	0	0.7	2.3	0
	Sellafield—Drigg	2	0.9	1.8	0	1.4	2.7	0	0	—	—
	Ravenglass—Eskmeals	1	0	—	—	1.9	—	—	0.6	—	—
	Little Ross	2	3.2	6.3	0	0.7	1.3	0	0	—	—

N, No. of trawls in which more than twenty fish were taken. * Including hauls by Danish seine net.

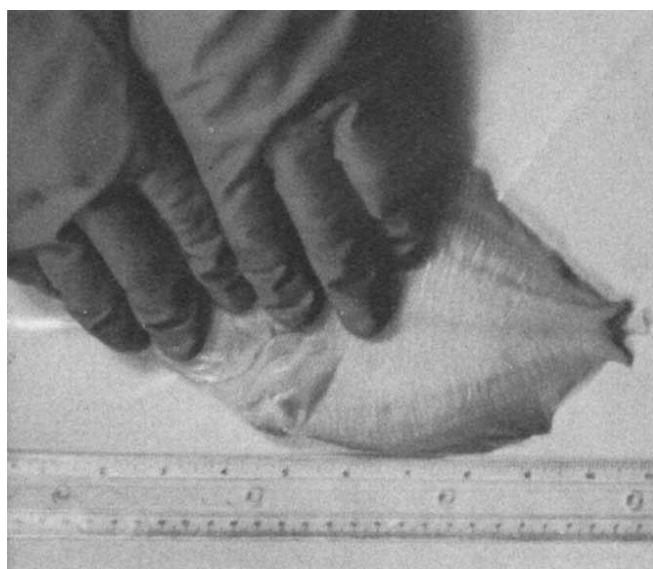


Fig. 5 Fin damage of plaice.

1969⁶. Some of these materials—the polychlorinated biphenyls (PCBs)—may, in sub-lethal doses, render treated animals more susceptible to the onset and influence of disease⁷. The absence of disease in young fish taken from the fishing and nursery grounds of the Solway suggests that the event which caused the diseased state occurred while the adult fish were in deeper water during the winter. So far, however, there is insufficient evidence to be certain whether the increased incidence of the diseases noted in 1971 is the result of an outbreak of epidemics of purely biological origin or if the dumping of toxic wastes is responsible. The histology of these diseases is under investigation and will be reported later.

E. J. PERKINS
J. R. S. GILCHRIST
O. J. ABBOTT

University of Strathclyde,
Marine Laboratory, Garelochhead, Dunbartonshire

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¹ Weissenberg, R., *Ann. NY Acad. Sci.*, **126**, 362 (1965).

² Wellings, S. R., *Ann. NY Acad. Sci.*, **126**, 479 (1965).

³ Wellings, S. R., *J. Nat. Cancer Inst.*, **33**, 991 (1965).

⁴ McArn, G. E., *J. Nat. Cancer Inst.*, **41**, 229 (1968).

⁵ Mann, H., *Ber. Dt. Wiss. Komm. Meeresforsch.*, **21**, 1, 219 (1970).

⁶ Bourne, A., *New Scientist*, **44**, 292 (1969).

⁷ Friend, M., and Trainer, D. O., *Science*, **170**, 1314 (1970).

Choline, One of Two Fungal Growth Stimulants in Anthers responsible for the Susceptibility of Wheat to *Fusarium graminearum*

AN explanation in nutritional terms of host, organ or tissue specificity of parasitic microorganisms has not received much attention although in some widely differing systems nutritional factors are clearly of paramount importance. Erythritol, asparagine and myoinositol are growth factors required by *Brucella abortus*, Brazilian strains of *Pasteurella pestis* and *Rhodosticta quercina* respectively. The presence or absence of these factors in their respective hosts, domestic animals, guinea-pigs and plum trees, determines their susceptibility or resistance to the appropriate parasite¹⁻³. We report a fungal growth stimulant determining organ specificity of a fungal pathogen of plants.

Fusarium graminearum causes headblight in wheat and other cereals. Partially extruded anthers promote the invasion of wheat heads by *F. graminearum*, an effect determined by the fungal growth stimulating properties of two compounds in anthers⁴, one basic and the other neutral⁵. The basic component has been isolated using the bioassay previously described⁶ to monitor the fractionation, and identified as choline.

Anthers of wheat (cv. 'Troll') were collected from a field with an especially adapted vacuum cleaner powered by a portable generator, and were stored at -20°C until used. The amount collected (160 g) was considered insufficient for experimental work leading to isolation of the active principles. As possible alternative sources for large scale preparation, whole wheat heads, whole grain and wheat germ were assayed for growth stimulating activity⁶; they contained 20, 6 and 30 U/mg respectively, compared with 50 and 130 U/mg for field-collected anthers and those dissected from greenhouse grown plants. Because the most active alternative to anthers was wheat germ, electrophoretic mobilities on paper of the growth-stimulating components in extracts from this source and from anthers were compared. Two zones of activity for each extract were recorded in buffers at pH 2, 3.4, 6.2 and 12.0. In all runs the results indicated that the basic and the neutral factor⁵ from each source were identical.

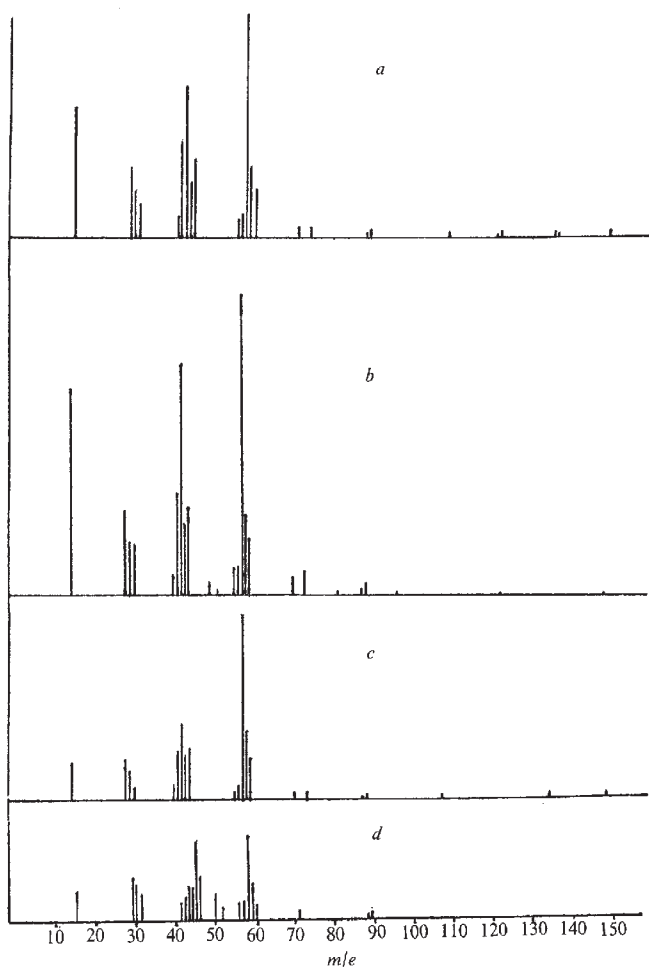


Fig. 1 Simplified mass spectra of basic growth stimulating factors: *a*, crystalline material from wheat germ; *b*, choline acetate; *c*, crude anther extract; *d*, crystalline material from anthers.

The isolation procedure for the basic factor was worked out with wheat germ and then applied on a small scale to anthers. The material was extracted in methanol, evaporated to dryness and the residue treated with a 1:1 ether:water mixture. The aqueous phase was run on to a column of 'Dowex' 50×8-200 in the H^+ form and washed with water until the effluent was free of sugars (thymol test). Elution with 0.5 M ammonium acetate, pH 6.5, separated the two factors in small batches but not in large batches. Separation in the latter instance was achieved by the use of another 'Dowex' 50×8-200 column in the NH_4^+ form which did not retain the neutral factor; the basic factor was eluted in ammonium acetate buffer. In each case ammonium acetate was sublimed from fractions containing the basic factor⁷ and the residue dissolved in a small volume of ethanol. An equal volume of ether was added and the solution acidified with concentrated HCl. An inactive white precipitate was centrifuged off and the active supernatant converted back to the acetate by absorption on a small column of 'Dowex' 50×8-200 (H^+) and elution with ammonium acetate buffer. After removal of the buffer by sublimation the residue was taken up in a small volume of ethanol and crystallized by the cautious addition of ether and cooling to $-20^\circ C$.

The crystalline preparations from wheat germ and anthers had growth stimulating activities of 27,000 U/mg and 23,000 U/mg respectively; allowing for dilution by the agar medium in the assay plate these are equivalent to about 7 parts in 10^9 .

The mass spectrum of the crystalline basic growth stimulating factor from wheat germ was determined using an AEI MS9 mass spectrometer, at a temperature of $220^\circ C$. After the intro-

duction of the sample the ion current monitor rose and fell twice and the spectra obtained at these two maxima were markedly different. The first spectrum was substantially that of acetic acid while the second was consistent with that of an amine of low molecular weight. The difference in the two spectra suggested that the sample was undergoing thermal decomposition in the source before ionization and this was confirmed by observing the rise and fall of ion currents at the m/e values of 58 and 89. The ion current curve profiles were long and trailing rather than Gaussian in shape, which suggests that the process was not one of simple evaporation. The atomic constitution of several ions responsible for peaks in the second spectrum was then determined by precise mass analysis and the results obtained were: 89.083127- $C_4H_{11}NO$, 71.073366- C_4H_9N , 58.062650- C_3H_8N . A small sample was then equilibrated with heavy water before evaporation into the ion source and the mass spectrum record revealed the incorporation of one deuterium atom only into the sample molecule, the peak at m/e 89 moving to m/e 90. This suggests that the oxygen atom is involved in a hydroxyl rather than an ether or a carbonyl group. Treatment of a small sample with nitrous acid produced no change in the mass spectrum, indicating the absence of primary or secondary amino groups. The spectrum (Fig. 1a) was therefore that of a tertiary amine containing a hydroxyl group and was similar to that obtained from dimethylaminoethanol. This latter species was being produced by the decomposition of the original sample and the identification of acetic acid and the existence of small peaks in the original spectrum, due to methyl acetate, suggested that the basic factor was choline acetate. This was confirmed by the similarity of the spectrum to that of a pure sample of choline acetate (Fig. 1b). A sample of crude basic growth stimulating factor obtained from anthers was also examined; the mass spectrum (Fig. 1c) is substantially the same. The small mass peaks at the high mass end of the spectrum disappear when the material is purified, and the mass spectrum of the crystalline material from anthers (Fig. 1d) is almost identical to that of choline acetate and the wheat germ extract.

Choline (BDH Chemicals, Ltd, Poole, Dorset) assayed as the chloride had 26,000 growth stimulating units/mg. Further corroboration that the basic factors in wheat germ and anthers were choline was provided by co-electrophoresis in buffers at pH 1.9, 3.4, 6.5 and 12.0, co-chromatography in two solvents and a positive reaction with Dragendorff reagent⁸. Determinations of choline concentration in extracts of different parts of wheat heads and plants are in progress.

It is possible that the neutral factor may be betaine and the purification of this compound is in progress.

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R. N. STRANGE

Department of Botany and Microbiology,
University College London,
Gower Street, London WC1E 6BT

H. SMITH

Department of Microbiology,
University of Birmingham, Edgbaston, Birmingham 15

J. R. MAJER

Department of Chemistry,
University of Birmingham, Edgbaston, Birmingham 15

Received March 30, 1972.

¹ Smith, H., Williams, A. E., Pearce, J. H., Keppie, J., Harris-Smith, P. W., Fitzgeorge, R. B., and Witt, K., *Nature*, **193**, 47 (1962).

² Burrows, T. W., and Gillet, W. A., *Nature*, **229**, 51 (1971).

³ Lukezic, F. L., and DeVay, J. E., *Phytopathology*, **54**, 697 (1964).

⁴ Strange, R. N., and Smith, H., *Physiol. Plant Path.*, **1**, 141 (1971).

⁵ Strange, R. N., and Smith, H., *J. Gen. Microbiol.*, **63**, x (1970).

⁶ Strange, R. N., and Smith, H., *Trans. Brit. Mycol. Soc.*, **56**, 485 (1971).

⁷ Hirs, C. H. W., Moore, S., and Stein, W. H., *J. Biol. Chem.*, **195**, 669 (1952).

⁸ *Data for Biochemical Research* (edit. by Dawson, R. M. C., Elliott, D. C., Elliott, W. H., and Jones, K. M.) (Clarendon Press, Oxford, 1969).

Isolates of *Puccinia striiformis* collected in England from the Wheat Varieties Maris Beacon and Joss Cambier

AN isolate (WYR 69/10) of *Puccinia striiformis* Westend. (yellow rust) was collected from a crop of the winter wheat variety Maris Beacon in 1969 and found to represent a hitherto undescribed physiological race. This has been designated race 3/55D¹ and as race 104E137 according to a new system of nomenclature². Among the varieties susceptible at the seedling stage to this isolate were Maris Beacon and Joss Cambier. In field trials in 1970 and 1971 small scale epidemics of yellow rust were induced, using isolate WYR 69/10; Maris Beacon became heavily infected with yellow rust but very little infection developed on Joss Cambier.

In 1971, when Joss Cambier occupied more than one-third of the acreage under winter wheat in the United Kingdom, there were reports of widely scattered and sometimes severe attacks of yellow rust on commercial crops of this variety (R. C. S. Macer, personal communication). Isolates of *P. striiformis* were received at the Plant Breeding Institute in Cambridge from two severely infected crops of Joss Cambier, the first (WYR 71/2) collected near Banbury, Oxfordshire, the second (WYR 71/13) supplied by the National Institute of Agricultural Botany and originally collected near Swindon, Wiltshire.

These two isolates from Joss Cambier were tested for their reactions with seedlings of wheat varieties, and were sufficiently similar to WYR 69/10 to be classified as race 104E137. In a series of tests in the glasshouse of more than forty wheat varieties, however, isolate WYR 69/10 gave slight but consistent differences from isolates WYR 71/2 and WYR 71/13 in reaction types induced on the varieties Hybrid 46, Joss Cambier and Heines VII. On these varieties the frequencies of occurrence of reaction types 3 (much sporulation with slight chlorosis), 3+ (much sporulation with barely detectable chlorosis) and 4 (much sporulation with no chlorosis) were as shown in Fig. 1.

To test whether these small differences could be expressed in a more definitive way, and in a way relevant to the development of the disease in the field, an experiment was designed to measure the weight of spores produced by seedlings of the varieties Hybrid 46 and Joss Cambier when infected with each

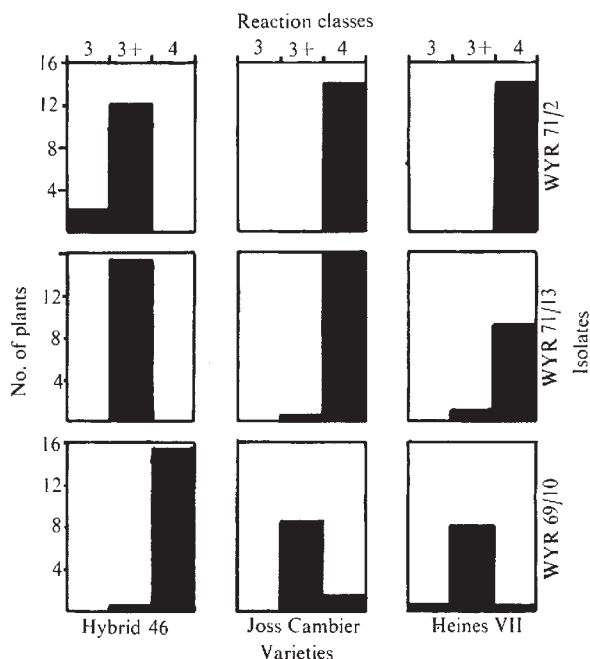


Fig. 1 Numbers of seedlings of three wheat varieties, infected with three isolates of *P. striiformis*, classified into each of the three reaction type categories 3, 3+ and 4. Total from two tests.

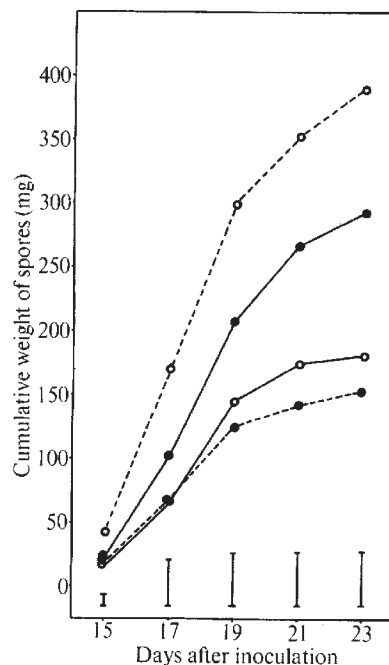


Fig. 2 Cumulative weights, in mg, of uredospores of two isolates of *P. striiformis* collected from 100 seedlings of wheat varieties Hybrid 46 (○) and Joss Cambier (●). Vertical bars represent l.s.d.s for treatment means at $P=0.05$. ---, Isolate WYR 69/10; —, isolate WYR 71/2.

of the two isolates WYR 69/10 and WYR 71/2. Batches of 700 seedlings of each of these two wheat varieties were inoculated, when their first leaf was fully expanded, with uredospores of each of the two isolates. The seedlings were sealed in polythene bags and placed in a refrigerator at 7° C for 36 h to permit infection, after which they were returned to a glasshouse bench with supplementary illumination from fluorescent tubes and a daylength of 18 h. The experiment was laid out in seven randomized blocks, each of four plots of 100 seedlings. Sporulation commenced approximately thirteen days after inoculation. Spores were collected from the seedlings on five occasions using a "cyclone" spore collector³, and the product from each plot of 100 seedlings was weighed immediately after collection. Collections were discontinued when the seedling leaves had shrivelled.

The cumulative weight of spores collected from 100 seedlings of each variety, infected with each isolate of *P. striiformis*, is illustrated in Fig. 2. There was a very significant interaction ($P<0.001$) between isolates and varieties at each spore collection. Isolate WYR 69/10 produced more than twice the weight of spores on Hybrid 46 as on Joss Cambier, and isolate WYR 71/2 produced almost twice the weight of spores on Joss Cambier as on Hybrid 46, which indicates that isolate WYR 69/10 had a greater reproductive rate on seedlings of Hybrid 46 than on seedlings of Joss Cambier, and isolate WYR 71/2 had a higher reproductive rate on seedlings of Joss Cambier than on seedlings of Hybrid 46. Furthermore, Joss Cambier produced twice the weight of spores with isolate WYR 71/2 as with isolate 69/10, and this may be related to the greater susceptibility of Joss Cambier in the field to isolate WYR 71/2.

These results demonstrate a sensitive method for assessing the interaction of isolates of *P. striiformis* with seedlings of wheat varieties and show that the attacks of yellow rust on Joss Cambier at Banbury and Swindon, from which isolates WYR 71/2 and WYR 71/13 were taken, were due to forms of *P. striiformis* which were genetically distinct from the isolate WYR 69/10 obtained from Maris Beacon. Although all three isolates were classified as belonging to the same physiological race when standard techniques were used, the difference between them is probably sufficient to influence significantly their epidemiology in the field. It is suggested that isolates

which resemble WYR 69/10 in their interactions with differential varieties should be classified as race 104E137 type 1 and those resembling WYR 71/2 and 71/13 as race 104E137 type 2. Such genetic adaptations in the pathogen, which have the effect of increasing the rate of sporulation, could result in more rapid spread of the disease on certain varieties.

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R. JOHNSON
A. J. TAYLOR

*Plant Breeding Institute,
Trumpington, Cambridge*

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¹ Chamberlain, N. H., Doodson, J. K., and Johnson, R., *Pl. Pathol.*, **20**, 92 (1971).

² Johnson, R., Stubbs, R. W., Fuchs, E., and Chamberlain, N. H., *Trans. Brit. Mycol. Soc.* (in the press).

³ Tervet, I. W., and Cassell, R. C., *Phytopathology*, **41**, 286 (1951).

Female Homosexuality

ALTHOUGH the cause of female homosexuality is not fully understood¹, it is usually thought to have a psychological origin; however, an organic cause has not been excluded. Studies have been carried out in both male² and female³ homosexuals; as these results are not conclusive, we have studied a group of female homosexuals with this in mind. The subjects were forty-two volunteers who were members of a lesbian organization, whose ages ranged from 22 to 55 years with a mean of 36 years.

The following enquiries and investigations were carried out. A medical history was taken, with emphasis on obstetric and gynaecological features, such as age at menarche, frequency of menstruation, and so on; an external physical examination was carried out; a 24 h urine sample was collected with the day of the menstrual cycle, and any concurrent drug therapy noted, and analysed for the following constituents using standard methods: oestrone, oestradiol, pregnanediol, pregnanetriol, 17-hydroxycorticosteroids, 17-oxosteroids, testosterone and epitestosterone; any specimen yielding anomalous results was repeated. A buccal smear was taken, and karyotyping was performed if thought to be indicated. Somatotyping and anthropometric measurements were carried out, and thirty-seven of the volunteers were evaluated by the Eysenck personality inventory.

The results of these investigations were as follows. The history and physical examination revealed no significant abnormalities; secondary sexual characteristics and the external genitalia were normal. Hormonal concentrations in the 24 h urine samples were within the normal range, with allowance being made for any volunteers taking hormones; buccal smear examination also demonstrated the normal female pattern.

Seventeen anthropometric measurements were taken, and compared with those of a control group of similarly aged mothers of a sample of normal children. The lesbians were significantly greater in stature and shoulder width; but the latter difference disappeared when the difference in sheer size was allowed for. A discriminant score for androgyny⁴ showed a statistically significant though very minor difference of 1.4 units, the lesbians being greater in "masculinity"; but the difference between an average man and an average woman of this age on this score is 14 units. When size is allowed for, even this minor difference disappears. The somatotypes of the lesbians covered a wide range, and though we have no very suitable control group for this comparison, it is absolutely clear that there is no such thing as a lesbian physique. We have not been able to examine physique in relation to the sexual role adopted (even supposing this is invariant or meaningful) but the distribution shows no evidence of bimodality. About the only striking thing was the visual impression that the

subjects mostly looked older than their age and sometimes strikingly so. The age of recollected menarche was 13.4 yr, which does not significantly differ from that of the general population.

Thirty-seven patients were tested by the Eysenck personality inventory⁵, which measures two dimensions of personality, namely neuroticism (anxiety, restlessness, tension, vulnerability to stress) and extroversion (impulsiveness, sociability, empathy, gregariousness). The mean neuroticism score for the group was 13, and this is significantly higher than for a normal group. The mean extroversion score was 8.73 which is significantly low compared with a normal group, showing the lesbian group to be clearly dysthymic, that is, prone to anxiety and nervousness, with obsessive tendencies.

This study suggests that, apart from their sexual orientation, lesbians are more neurotic and less extrovert than average, but are apparently in all other respects normal. Female homosexuality may yet be found to have a significant physical cause, but this study failed to reveal any obvious organic difference between lesbians and heterosexual women.

A. J. EISINGER
R. G. HUNTSMAN
JENNY LORD

*St Thomas's and Lambeth Hospitals,
London SE1*

Guy's Hospital, London SE1

J. MERRY
P. POLANI

J. M. TANNER
R. H. WHITEHOUSE

*Institute of Child Health,
University of London*

*Clinical Chemistry Department,
Dundee University*

P. D. GRIFFITHS

Received March 9, 1972.

¹ Kenyon, F. E., *Brit. J. Hosp. Med.*, **3**, 183 (1970).

² Kolodny, R. C., Masters, W. H., Hendryx, J., and Toro, G., *New Engl. J. Med.*, **285**, 1170 (1971).

³ Loraine, J. A., Ismail, A. A., Adamopoulos, D. A., and Dove, G. A., *Brit. Med. J.*, **4**, 406 (1970).

⁴ Tanner, J. M., *Growth at Adolescence*, second ed. (Blackwell Scientific Publications, Oxford, 1962).

⁵ Eysenck, H. J., and Eysenck, B. G., *Manual of the Eysenck Personality Inventory* (Univ. of London Press, 1964).

Herbal Medicines to Avoid

IN East Africa, certain rare tumours, oesophageal, nasopharyngeal, liver, Burkitt lymphomas and the like, are concentrated in distinct localized pockets¹. The reason for this and the aetiology of these tumours are not known. More than one factor may be involved. Such agents as viruses, or microbial toxins present in food, however, are not easy to avoid. Is there some relation between these tumours and the plants that grow locally and are eaten as food or as medicine?

Poisonous plants are usually known as such by the local people; nobody, however, could be aware of the hazard of plants which do not show immediately toxic effects, but which act insidiously and can cause chronic disease and eventually death after a long latent period even with a single dose. Such insidious action has only recently come to be recognized, and can only be detected by screening the plant materials in long term experiments on animals including pregnant and lactating females². The vegetation of East Africa is known to be rich and varied, and some systematic studies on the distribution of various species have been published or are in preparation³. The type of vegetation depends on altitude, climatic and soil conditions, and these factors can affect the content and the particular structures of toxic plant constituents, even in closely related species. There are more than 200 species of *Crota-*

laris and large numbers of species of the genera *Senecio*, *Heliotropium*, *Cynoglossum* and *Trichodesma* (Dr J. B. Gillett, personal communication). More than 100 pyrrolizidine alkaloids have been identified in these plants⁴. Not all are toxic, but their biological effects cannot be fully known until they have been rigorously tested.

Pyrrolizidine alkaloids were investigated pharmacologically about 25 years ago by Harris *et al.*⁵ and found to have no significant medicinal value. Many of the alkaloids have, however, hepatotoxic and carcinogenic action^{1,5,6}, and a single dose may be sufficient to induce tumours in rats⁷. An oral dose of the order of LD₅₀₋₆₀ will cause liver lesions in some of the rats resembling hepatitis with jaundice; other treated rats that survive longer develop more chronic effects, like liver cirrhosis or primary liver tumours, which take time to "ripen", and become apparent after long latent periods. The end effects depend on the structure of the alkaloids, on the size of the dose², on the dosage schedule⁶ and in particular on the susceptibility of the individual—a function of age⁸, sex, diet¹ and so on. Pyrrolizidine alkaloids can affect lung, heart, vascular system, pancreas, brain, and kidney^{1,9-11} as well as the liver and other organs.

In the industrially less developed countries, which still rely mostly on natural products for their traditional medicines, liver disease and primary liver tumours are common. Moreover, liver lesions are found in young children, either in the form of liver cirrhosis or kwashiorkor or marasmus which are often thought to be due to malnutrition. Most cases of true malnutrition recover or improve on an appropriate diet but about 20% of hospital admissions of children suffering from "malnutrition" (often still on the breast) die there. The proportion of incurable "malnutrition" remains almost the same in spite of the various nutritional regimens that have been tried in the past 30 years.

Incurable cases of marasmus and liver cirrhosis are probably of toxic origin, the children having been exposed to hepatotoxins, whether taken by their mothers during pregnancy or lactation, or as remedies for childhood disorders. It is significant that measles—a comparatively mild disease in western countries—leads often to "malnutrition" of the severe type in African children. Could this be the consequence of treating measles by the application of fresh juice from *Crotalaria* to the affected skin?¹²

If so, it should be possible to stop people from using as medicines particularly dangerous plants. The decrease of the incidence in children of "veno-occlusive" liver disease in Jamaica following a successful educational campaign against the use of the toxic *Crotalaria fulva* L in "bush teas" (Professor G. Bras, personal communication) is an example. *Crotalaria fulva* L contains fulvine, a pyrrolizidine alkaloid.

In young children suffering from liver disease or various tumours, there is some chance of tracing the aetiological factors. Mothers may remember any remedies they used themselves during pregnancy, parturition or lactation and which they gave to the child. A few well documented cases could give a clue which would be very important for the understanding of the aetiology of such disorders, not only of children, but probably of adults as well, opening up possibilities of prevention.

The difficulty in obtaining reliable information from the mothers of sick children, however, cannot be underestimated. Most will not admit having used any of the traditional remedies either for themselves or for the children. Only when several plants, some toxic and some innocuous, are shown to a group of mothers of both healthy and sick children is it usually possible to start discussion of the plants and the conditions for which these would be used. Using interpreters, much patience and observation, the true situation can be understood and the plant or plants used identified.

The realization that her child's illness might have been caused inadvertently by her own actions can be the cause of great anguish for the mother, as I witnessed in one woman, mother

of a 6 months old child suffering from severe marasmus. Her depression and distress were due to the fact that she believed that the child developed the illness after she had taken a plant medicine. She seemed to recognize a *Trichodesma* as the one she used. The child died two days later in spite of an intracranial drip and other modern attempts to treat this usually incurable condition. The mother of a child with kwashiorkor explained that the child had measles and had been treated with a herbal medicine brought by its grandparents from their village, but the identity of the plants could not be established. The possibility that toxic plants might sometimes be included in childhood "medicines" receives support from the report of two fatal cases of children treated with some unidentified herbs¹³. Direct evidence that *Senecio*, *Crotalaria*, *Cynoglossum* and so on are recommended by local "herbal doctors" was obtained from a medicine man in a village between Addis Ababa and Menagasha State forest to whom I showed certain species of these plants (collected in the forest with the help of the botanist, Mr M. Gilbert), some of which were known to be hepatotoxic¹⁴. The medicine man explained that he prescribes pounded roots as "antidote" for stomach trouble or other illnesses including those induced by the "evil eye". Thanks to Mr J. E. Mills, the representative of the British Council in Addis Ababa, and his team, the collection of the plants was filmed as well as interviews with the medicine man and with various patients including mothers of children suffering from kwashiorkor. This film with an additional part showing the testing of such plants and their effects in experimental white rats has been prepared at the MRC Toxicology Unit in Carshalton, and is available from the Medical Research Council in London.

In each of the East African countries visited, there is at present great interest in their national traditional herbal medicines, especially as regards their toxicological and pharmacological evaluation. Such studies, which are now being initiated and should include long term testing in animals, would make it possible to identify the plants that can be harmful and those that are medicinally valuable—so that appropriate preventive measures can be taken against the use of the dangerous ones. It is hoped that in time this will lead to the decrease in the incidence of some of the incurable chronic diseases and cancers which, besides causing much human suffering, are a considerable drain on the economic resources which developing countries can ill afford.

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R. SCHOENTAL

MRC Toxicology Unit,
Medical Research Council Laboratories,
Woodmansterne Road,
Carshalton, Surrey

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¹ Hutt, M. S. R., and Burkitt, D., *Brit. Med. J.*, **2**, 719 (1965).

² Schoental, R., *Cancer Res.*, **28**, 2237 (1968).

³ *Flora of Tropical East Africa* (71 parts already available), Government Printer, PO Box 30128, Nairobi, Kenya, or Government Bookshop, London, SE1.

⁴ Warren, F. L., in *The Alkaloids* (edit. by Manske, R. H. F.), **12**, 245 (Academic Press, London, 1970).

⁵ *Research Today*, **5** (Eli Lilly, Indianapolis, 1949).

⁶ Harris, P. N., and Chen, K. K., *Cancer Res.*, **30**, 2881 (1970).

⁷ Schoental, R., and Bensted, J. P. M., *Brit. J. Cancer*, **17**, 242 (1963).

⁸ Schoental, R., *Nature*, **227**, 401 (1970).

⁹ Kay, J. M., and Heath, D., *Crotalaria spectabilis—The Pulmonary Hypertension Plant* (Thomas, Springfield, Illinois, 1970).

¹⁰ Schoental, R., Fowler, M. E., and Coady, A., *Cancer Res.*, **30**, 2127 (1970).

¹¹ McLean, E. K., *Pharmacol. Rev.*, **22**, 429 (1970).

¹² Watt, J., and Breyer-Brandwijk, M. G., *The Medicinal and Poisonous Plants of Southern and Eastern Africa* (Livingstone, Edinburgh, 1962).

¹³ Bwibo, N. O., *Brit. Med. J.*, **4**, 601 (1969).

¹⁴ Schoental, R., and Coady, A., *East Afric. Med. J.*, **45**, 577 (1968).

Chesowanja Australopithecine

SZALAY, in a critical note¹ on my description of the Chesowanja australopithecine², presents a reconstruction of the cranium. He postulates not only the lateral pressure that I assumed had acted on the specimen but also that the palate was pushed inferiorly and anteriorly, opening out a wedge in the maxillary region. Using this postulate he then makes a reconstruction by graphically removing this wedge and bringing the cut faces together.



Fig. 1 Right lateral view of KNM-CH-1 showing line of section seen in Fig. 2. Matrix-filled areas and the central cavity of the maxillary air sinus have been shaded in. The pterygomaxillary fissure is seen as a sinuous matrix-filled groove posterior to the line of section.

In Figs. 1 and 2, I show here that there is no maxillary bone missing from the area shown by Szalay as a wedge in his Fig. 1. The section along the line A-C was made from a wax impression of the surfaces and careful checking with microscopic control on the original. From the medial crest bordering the inferior orbital fissure at A, continuous bone can be followed across the surface to the broken alveolar margin of the unerupted M³. The join between the two main fragments of the maxilla is marked on the surface by a very fine crack (B) on a single bone plate (the pieces were glued together before cleaning). No bone is missing from this area and in fact the cracked plates are seen to have overridden each other a little, a sign of flattening of the gently-domed maxillary surface by lateral pressure. The "faulting" of the typically thin bone on this infratemporal part of the maxilla indicates crushing by lateral pressure but none of these "faults" has a throw of more than 1 mm.

It is instructive to note that the depressed area lies in a position where it might have been caused by the folding of the zygomatic arch at its midpoint by lateral pressure. The margins of the pterygomaxillary fissure can be followed from the maxillary tuberosity to the infraorbital fissure and, on the broken faces of the two main parts of the specimen, the matrix-filled fissure can clearly be seen. The medial pterygoid plate is complete from the maxillary tuberosity to its fusion with the main mass of the sphenoid. All these observations show that Szalay is unjustified in graphically removing a wedge of cranium to make his reconstruction.

The use of the rameal height of the Peninj mandible to support the erroneous reconstruction is fallacious, for, after

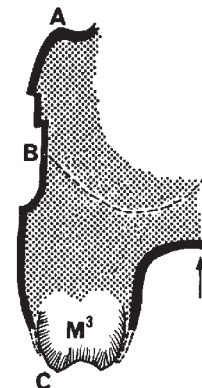


Fig. 2 Nearly coronal section through the right posterior maxillary region of KNM-CH-1. Cortical bone is shown black, the line of fracture through the specimen is shown as a curved, broken line and the mid-line of the palate is indicated by an arrow.

all, that mandible was sampled from an older population some 200 miles away. When Tobias and Clarke³ reconstructed a mandible for the Olduvai hominid 5 cranium, for instance, their value for rameal height exceeded that of the Peninj mandible quite considerably. It can be pointed out also that the position of the temporomandibular joint differs in Szalay's two figures, and yet the entoglenoid process is preserved in the specimen and should have allowed him to place his reconstructed condyle more exactly. In fact his Fig. 1 has the condyle in an illusorily anterior position, thus contributing to the peculiar appearance of the reconstruction, and in his Fig. 2 it has been moved posteriorly and nearer its correct place.

I gave three reasons for thinking that KNM-CH-1 had less post-orbital constriction than usually seen in robust australopithecines and only the second depends on the placing of the frontal fragment. This piece cannot be placed where Szalay would like it, for two reasons. First, when freshly cleaned with an air-brasive machine by Mr R. J. Clarke, the two pieces matched in fracture shape and fine detail at the position I showed in the photograph; and, second, the bone thicknesses are identical at the position I showed whereas the bone on the main fragment is no less than three times as thick as the bone of the posterior wall of the frontal sinus if Szalay's placement is attempted. The "median supraorbital depression" (*sic*) that Szalay reports is, in fact, a small "fault" on the specimen and can be demonstrated by a corresponding "fault" on the internal surface.

Szalay's veiled remark about the East Rudolf specimen KNM-ER-732 presumably implies that post-orbital constriction was not great in that specimen. I regret the implication that I made no mention of the East Rudolf specimen in order to mislead. That specimen has now been cleaned and details have been published^{4,5} and in fact the estimated minimum post-orbital width is almost exactly the same as in the complete cranium (KNM-ER-406) from East Rudolf, that is, about 60 mm.

Szalay disputes my evidence for enlarged middle and anterior cranial fossae in KNM-CH-1. Although I stand by my original statement, space considerations do not allow me to reply to this aspect here, but Professor Ralph Holloway is making an independent study of the endocranium and when that work and my own detailed description are published, I hope that Szalay's comments will be answered.

The sex of the specimen was not discussed in my preliminary report, but Szalay considers it to be female. Now that the East Rudolf specimen, considered to be an adult female, has been cleaned, a comparison can be made. An estimate of facial height that can be taken on all comparable specimens is the distance from the alveolar margin at M³ to the medial crest of the inferior orbital fissure. The dimensions for two undisputed male specimens are 62.0 mm (KNM-ER-406) and 68.0 mm (Olduvai hominid 5); for the presumed female cranium

(KNM-ER-732), 48.0 mm; and for the Chesowanja specimen, 67.0 mm. These figures would seem to speak for themselves, but a specimen from Swartkrans (SK 48) is estimated from a cast at 55.0 mm and there is evidence of larger specimens from that site (for example, SK 12). In spite of the fact that differences are to be expected in populations separated in time as well as space I think it fairly safe to take the opposite view to Szalay and say that on size alone KNM-CH-1 is most probably a male.

When my description of this important specimen is completed, the relationships between it and other australopithecines will, I hope, become clearer. Szalay's comments give me no reason to change the basically descriptive statements that I gave in the preliminary report.

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ALAN WALKER

Department of Human Anatomy,
University of Nairobi,
PO Box 30197, Nairobi, Kenya

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¹ Szalay, F. S., *Nature*, **234**, 229 (1971).

² Carney, J., Hill, A., Miller, J. A., and Walker, A., *Nature*, **230**, 509 (1971).

³ Tobias, P. V., *Olduvai Gorge*, **2**: The cranium of *Australopithecus* (*Zinjanthropus*) *boisei* (Cambridge University Press, 1967).

⁴ Leakey, R. E. F., *Nature*, **231**, 241 (1971).

⁵ Leakey, R. E. F., Mungai, J. M., and Walker, A. C., *Amer. J. Phys. Anthropol.*, **36**, 235 (1972).

GENERAL

Effectiveness of Combining Title Words and Index Terms in Machine Retrieval Searches

To determine the most effective method for searching the bibliographic data bases¹ available on magnetic tape, we have compared the differences in the citations retrieved when we searched on title words alone, on index terms alone, assigned from a controlled vocabulary by (presumably) knowledgeable indexers, and on both the title words and the index terms simultaneously. Previous studies²⁻⁵ have been mainly concerned with a comparison of machine retrieval and manual searches.

Our experiment was based on Volume 24 of Nuclear Science Abstracts (NSA) which contains about 53,000 citations; we used the generalized file-management system, Master Control⁶, which can operate in either an inverted or a linear search mode. The inverted mode uses a table composed of the unique vocabulary contained in one or more data elements, along with all record numbers in which each vocabulary word occurs. For example, an inverted table constructed on titles will have one entry for each unique word of every title in the data base, plus the record numbers in which each word was found. (In Master Control, a word is defined as any set of characters bounded on either side by a legal separator such as a blank, period, comma, colon, etc.) On the other hand, in a linear search mode the data element is compared with the profile word, character by character, which results in a prohibitively time-consuming process for large data bases.

We chose the inverted-table technique because of the large amount of data to be searched. Individual tables were constructed from the titles of the articles, NSA index terms, and titles and index terms combined. NSA index terms are controlled by the *Euratom Thesaurus*, as revised for NSA⁷.

We used two criteria in the study. First, the questions had to be of real interest to laboratory personnel. Some of the questions had actually been submitted by other members of the staff, to be run concurrently on the same data base on an SDI

basis. The others were especially constructed by the authors for this experiment. Second, citations obtained were to be considered good (or relevant) only if they actually pertained to the subject in question; otherwise, they were to be considered "false drops", regardless of the number of words matched between the profile and the citation.

For example, NSA 31371, "Radiolysis in Naphthalene Single Crystals", although matching its search profile exactly, is a "false drop", because it is about optical methods of observing radiation effects rather than the effects of radiation on optical properties.

The profiles actually used were then constructed by linking combinations of whole words, word fractions, phrases and synonyms, through both AND and NOT logic. Word fractions, which are designated by the term "Root", can be truncated at either end. For example, Root: *ion* will yield all words which include the group *ion*, such as *pion* and *ionizing*, while Root: **ion* will yield only words such as *ration*, *nation*, *pion*, that end in *ion*, and Root: *ion** will yield only words which begin with *ion*. The 25 profiles so constructed were then run on each issue of NSA against the three separate tables. Examples of the various types of strategies used in profile construction are shown in Table 1.

It will be noted that the overall precision, or amount of data retrieved per question, is quite good. This fact, however, is not really relevant to this experiment, because the questions were chosen to produce high precision. Although we can draw some subjective conclusions about the quality of NSA indexing, the real purpose of this study was to assess the relative merits of index terms, titles, and the combination of the two, for retrieval purposes. As one might expect, titles plus index terms generally returns more good retrievals than either titles alone or index terms alone, as is shown in Table 2.

As we can derive from Table 2, however—with the exception of one entry, "Quasilinear Theory of Plasmas", which is biased toward titles because of its construction—index terms returns from between 57% and 100% of the total number of relevant citations retrieved on titles plus index terms, with an average retrieval of 92%. Titles only, in contrast, returns between 6% and 96% of the total citations, with an average return of 31%.

Table 1 Examples of Profile Construction

Question	Search strategy
Interaction of Lasers and Plasmas	Root: Plasma PLUS Root: Laser
Monte Carlo Methods	Root: Monte PLUS Whole: Carlo
Information Retrieval	Whole: Information PLUS Synonym Root: Retrieval*, Keyw*, Title*
Penning Ion Sources	Synonym Whole: Penning, Penning* Type, PIG, PIG*, PLUS Synonym Whole: Ion, Ions, Source, Sources, Discharge, Discharges; REJECT Synonym Whole: Guinea, Swine
Equation of State	Synonym Whole: Equation of State, Equations of State
Channelling and Pair-Production as Tests of Quantum Electrodynamics	Synonym Whole: Channelling, Pair, Pairs, PLUS Root: Quantum
Quark Searches	Synonym Root: Quark*, SU-3, Fraction, PLUS Synonym Root: Search, Shower, Chamber, Sea*
Electron-Ring Accelerators	Synonym Whole: Electron Ring, Electron-Ring, ERA, Adge* ator, Collective Accelerator, Collective Accelerators, Collective Acceleration, Charged Ring, Self Focusing Ring
Effects of Radiation on Optical Properties of Crystals	Synonym Root: Radi, Ray, PLUS Synonym Root: Optic, Opac, Refrac, PLUS Synonym Root: Halide*, Hydride*, Crystal*

A comparison of the search times involved indicates that a single title index-term search takes between 5 and 40 s per issue, with an average time of about 25 s. Title-only searches take between 4 and 30 s, with an average time of about 22 s. An index term-only search, on the other hand, takes between 2 and 25 s with an average time of about 18 s. Thus, it is fairly clear that index term-only tables are a much more effective basis for retrieval than title-only.

Table 2 Master Control Special Study: Summary Sheet

Question	Titles plus index terms	Retrievals	
		Titles	Index terms
1. Interactions of Lasers and Plasmas	132	76	131
2. Monte Carlo Method and Applications	215	98	212
3. Poincare Groups	41	17	40
4. Plasma Transport	213	29	209
5. Radiation Effects on Immunological Processes	417	161	399
6. Information Retrieval	39	7	32
7. Penning Ion Sources	25	15	22
8. Equation of State	35	22	29
9. Relation Between Pair Production or Channelling and Quantum Electrodynamics	19 (3) *	1	16 (2)
10. Radioactivity in Food Chains	25	4	23
11. Electron Scattering by Metal Foils	22 (3)	8	21 (3)
12. Noble Metals in Plants and Animals	12 (2)	2	12 (2)
13. Quasilinear Theory of Plasmas	9	5	0
14. Quark Searchers	25 (2)	22	15 (2)
15. Polarized Targets	35 (2)	20	26 (2)
16. 3-Nucleon Problem	128 (1)	100	115 (1)
17. Ground-Water Transport of Radionuclides†	—	—	—
18. Radiation Effects on Optical Properties of Crystals	34 (9)	6 (1)	19 (5)
19. Feasibility and/or Economics of Fusion	10 (1)	5	6
20. Neutron/Photon Transport in Air	22 (3)	10	17 (3)
21. Relativistic Calculations	104 (7)	18 (1)	85 (4)
22. Effects of Nuclear Explosions on Crops	73 (27)	19 (4)	65 (21)
23. Radiation Effects on Human Behaviour	11 (7)	1	7 (3)
24. Radiation Doses and their Effects on Man	586 (25)	31	551 (24)
25. Electron-Ring Accelerators	47 (4)	28 (4)	41

* "False drops" in parentheses.

† Not included in summary.

What is the value of searching titles plus index terms as opposed to searching index terms alone—or alternatively, is the extra information obtained worth the extra cost in machine time and subject-specialist time to weed out the extra "false drops"?

On a strictly quantitative basis, this question is difficult to answer. A thorough study of all the profiles used and all the citations obtained has, however, convinced us that the answer is "yes" for the following reasons. Many citations that have excellent titles are either not indexed at all, or incompletely indexed, precisely because of the quality of the title: for example, the paper "CIRK—CTC's System for Information Retrieval from Keyboards" (NSA, 24, 53037) has no index terms. Some other examples of papers listed in NSA, 24 with excellent titles that were not retrieved on index terms only are: On the *Optical Absorption of X-ray-Irradiated NaClO₃ Crystals* (13967), *Equation of State of Detonation Products* (33618), *Extrapolation to Man of the Doses Required for Radiotherapy* (853), *Fallout Radiation and Crop Productivity* (12319).

Many relevant citations can be retrieved only through the logic of matching the profile partially from index terms and partially from titles: some examples are given below.

NSA abstract No.	Question No.	Title of article	Index term
3715	3	<i>Poincare Generators, Unitary Transformations and Helicity Representations</i>	<i>Group Theory</i>
23047	18	<i>Optical Properties of Dosimetric Crystals of Lithium Fluoride</i>	<i>Radiation Effects</i>
37605	9	<i>Measurement of Electroproduction of Muon Pairs</i>	<i>Quantum Electrodynamics</i>
50807	24	<i>Cytogenetic Effects of Low-Dose Internal and External Radiations</i>	<i>Man</i>

Many of the citations that cannot be obtained from index terms are not only relevant but also add a certain "richness" or directness to the subject in question, especially when the topic is very current and there is either no agreement on terminology or it has not yet become part of the control thesaurus.

Finally, special types of searches can be done on titles containing specific symbols, such as (n,γ), (d,α), etc.

Although a different set of profiles will obviously change both the results of individual questions and the average results of the study, we believe that the relative ratio of index term to title retrieval, about 2.5 to 1, would be maintained. One indication of this is that if the question on Radiation Doses, the single largest contributor, is deleted, then the average retrieval results are 38% for titles and 92% for index terms.

Of the total citations retrieved from simultaneously searching both titles and index terms, 92% were retrievable from index terms alone, while only 31% were retrievable from titles alone. From our results, we believe that this ratio would remain approximately the same if the experiment were repeated over a long period, using only the questions actually submitted by laboratory scientists; unfortunately, the cost of computer time for processing each question three times would prevent us from running a sample large enough to prove this assumption.

We believe that the greatest significance of our study is that it demonstrates a practical method for obtaining statistically significant amounts of hard data on the effectiveness of searching machine-readable bibliographic data bases. We have shown that, in the field of nuclear science and technology, there seems to be a high probability that retrieval on the basis of titles alone is inadequate. We conclude that an extension of our technique, using larger samples and other data bases, would both produce numerical comparisons of quality of indexing among data bases and provide a way to test alternative methods of constructing indexes and assigning titles.

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H. LEONARD FISHER
DENNIS R. ELCHESON

Lawrence Livermore Laboratory,
University of California,
Livermore, California 94550

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- Carroll, K. D., *Survey of Scientific-Technical Tape Services*, AIP ID 70, ASIS SIG/SDI 2 (1970).
- Abbot, M. T. J., Hunter, P. S., and Simkins, M. A., *ASLIB Proc.*, **20**, 129 (1968).
- Lancaster, F. W., *Amer. Doc.*, **20**, 119 (1969).
- Angstadt, H. P., *J. Chem. Doc.*, **10**, 277 (1970).
- Lynch, J. T., and Smith, G. D. W., *Nature*, **230**, 153 (1971).
- Hampel, V. E., and Wade, J. A., *Proc. Amer. Soc. Inform. Sci.*, **6**, 159 (32nd Annual Meeting, San Francisco, 1969).
- Euratom Thesaurus*, as revised by the US Atomic Energy Commission, TID-25439 (1970).

BOOK REVIEWS

Calculus of Optimism

The Doomsday Syndrome. By John Maddox. Pp. viii+248. (Macmillan: London and Basingstoke, June 1972.) £2.95.

The Mature Society. By Dennis Gabor. Pp. 208. (Secker and Warburg: London, May 1972.) £2.75.

MANY ecologists are, with ample justification, indignant about the sensational exploitation of ecology by some of their colleagues, who publish naïve predictions about the future of the environment, resting on arguments which no scientific journal would accept. Sensationalism pays: their writings are read by the public with a relish hitherto reserved for books about crime or sex. Hard on the heels of the notorious *Blueprint for Survival* came *The Limits to Growth*, which enlists the authority of a computer to predict a collapse of western society unless birth rate is equated to death rate and investment to obsolescence. In both these works the conclusions are widely quoted; the assumptions are scarcely questioned.

But it is the assumptions which matter. Much of this doomsday literature depends for its impact on an uncritical selection of facts, coloured by expressions such as "ecological suicide", "environmental catastrophe", and so on; and the gist of its message is pessimism: its recipe for survival would entail a revision of values which would so erode personal freedom that for many people survival would not be worth while. For two reasons it is difficult to criticize this pessimism: first, because opinion is so polarized about it that criticism is liable to be dismissed as complacency; and second, because reasoned argument in a low key about the future makes dull reading beside the strident self-assurance of prophets of doom.

The books by John Maddox and Dennis Gabor both advocate optimism about the future of mankind, and they are a welcome and necessary antidote to those who cry out that the end of the world is at hand. But there their similarity ends. Maddox

examines critically the evidence on which charismatic writers, like Carson, Ehrlich, and Commoner, rely to support their forebodings. His theme is that, yes, man is using the resources of the Earth and changing the environment; but so what? The resources are so great and the man-made changes so trivial compared with natural changes (such as volcano eruptions and ice ages) that there is no cause for alarm, nor for an abrupt break in the style of living. He questions the common assumption that population will outstrip food supply. He does not share the common anxiety that we shall exhaust our reserves of essential metals, or if we do, that we shall not be able to get along quite well without them. He doubts the danger associated with the use of pesticides. His essential argument is that technology is not to be blamed for our present discontents or our apprehensions about the future: history demonstrates that technological innovation and economic growth have brought benefits to society undreamed of a century ago, and there is no reason to believe that the foreseeable future will differ in this respect from the recent past.

The book is a welcome and timely exposure of the fallacies in the propaganda of conservationists. But it is not quite the book which is needed to lower the temperature of argument over this important issue, for it too is a product of the polarization of opinion which it so rightly criticizes. In his zeal to confound the prophets of doom, Maddox himself occasionally adopts their technique. Where they wilfully exaggerate future hazards, he seems wilfully to underestimate them. For instance, he says "there is *no doubt* that the world could support 7,000 million people *or even twice* that number". (The italics are mine.) This is a pity; what is needed is a severely clinical analysis of the doomsday syndrome, for this would demonstrate that we simply do not have the data to make long-range predictions, bright or gloomy, about the future of the environment; any more

than we can make long-range predictions about much simpler phenomena, like the weather or the stock market.

But the message of this book is sound. The institutions of society do not adapt themselves in anticipation of social needs, only in response to them. The only practical blueprint for adaptation will be a bill of quantities laboriously prepared by social engineers, not a burst of doomsday rhetoric.

Gabor enquires more deeply into the doomsday syndrome. He believes, as firmly as Maddox does, that science and technology will continue to benefit mankind. But unlike Maddox, who sees no unique significance in the present human condition, Gabor regards the present as a climacteric in the history of industrial societies; a moment when affluent nations are realizing that they will, sooner or later, have to adapt themselves to asymptotic growth: in population, in production, in standard of living. But the prime problem of asymptotic growth is not shortage of food or metals, nor is it urban decay or pollution: it is how to deal with the psychological consequences of affluence. Gabor is not concerned as to whether the consumer society can be sustained, but whether it will be worth sustaining in the minds of those who live in it.

The epidemics which may wreck our society are, in his view, not physical but mental. He sees the symptoms in student unrest, absenteeism in industry, and the alarming spread of crime and drug addiction; all a malaise of affluence, not poverty. His themes are that "man is wonderful in adversity, weak in comfort, affluence and security", and that "man does not appreciate what he gets without an effort". So Gabor's hope for the future is that it will provide some tension and conflict, and require some effort for survival. In this hope he is not likely to be disappointed. His recipe for survival is to recommend that education—the apprenticeship to living—should be exacting and "hard", and that governments have to carry out an "ordered retreat", from a society which

idolizes growth to a mature society; by which he does not mean stopping growth here and now, but working toward what he calls a "gentle saturation".

Gabor does not underestimate the magnitude of the social changes which are needed, but unlike those afflicted with the doomsday syndrome, he does not want to force the pace of change. "Stopping the machinery" of society is, he says, "not enough; any fool can do it and there are enough fools busy doing it." He puts in a plea for the value of computer simulation. Well and good; but it is a pity that such a distinguished scientist as Gabor apparently accepts, without challenge, the naive and sometimes downright nonsensical inputs included in the exercise sponsored by the Club of Rome. The computer is a very promising instrument for planning and prediction; but if you feed it with nonsense it will excrete nonsense. It will be a great misfortune if the use of computers as an aid to the prediction of social change is discredited.

ERIC ASHBY

A Model of Doom

The Limits to Growth. By Donella H. Meadows, Dennis L. Meadows, Jørgen Randers and William W. Behrens III. Pp. 250. (Universe Books: New York, 1972.) \$2.75.

IN *The Limits to Growth* Professor Meadows and his colleagues describe a computer simulation of certain global physical stocks, namely population, pollution, land, agricultural, industrial and service capital, and non-renewable resources, and the flows associated with them in the period 1900–2100. This book portends disaster from which only a policy of zero growth will save us. The population vector distinguishes three age groups, the other variables apparently being scalars. These variables are linked by equations, with coefficients representing what the authors believe to be the important effects.

Professor Meadows says that every assumption made is written in a precise form so that it is open to inspection and criticism by all, though unfortunately not in *The Limits to Growth*. This is a popular account of the model, the technical report on which is not yet available. The book is clearly written, however, and a sincere attempt is made to deal with the issues honestly, revealing rather than obscuring the difficulties. The methodology is familiar from Professor Forrester's books *Industrial Dynamics*, *Urban Dynamics* and *World Dynamics*. Professor Meadows does not follow Professor Forrester in ignoring empirical evidence and relying

only on his intuition. He draws on whatever estimates he has been able to find of specific effects, such as the relation between GNP *per capita* and steel consumption. Here, for example, he uses a very rough fit of a logistic curve to data on steel from a cross section of countries in 1968, to justify similar global behaviour on all resources over time, thus taking a very restricted view of changes in technology. Where he has not been able to find an estimate such as the effect of pollution on life expectancy he has made it up, in this case choosing that relation which shows the most apocalyptic effect—namely a negligible effect until high levels of pollution are reached, when the effect increases rapidly.

Overall, the treatment of population builds in persistent tendencies to exponential growth in developing countries rather than a large stepwise growth such as is nearing completion in the developed countries. On resources the book does not always distinguish clearly between manufacturing output, tonnage of raw materials absorbed, and sometimes even "economic growth", thus denying itself the tools to analyse effectively the problems and possibilities of technical change, substitution, and re-cycling processes. The cost of dealing with pollution is exaggerated.

The model is not estimated by a statistical analysis of data, even where, as in the case of population, a good deal of data is available. Professor Meadows justifies this course of action by arguing that the model is not intended as a forecasting model, but as a demonstration of the dynamic behaviour to be expected. He demonstrates the sensitivity of this behaviour to changes in coefficient values and some changes in model structure, producing variations on the theme of disaster. But provided poles are kept well clear of stability boundaries in variations of coefficients and structure, the dynamics of any model are to the uninitiated surprisingly insensitive to "assumptions". The test is whether the broad structure is right. In estimating systems, even where albeit noisy data are available, it is often exceedingly difficult to test statistically whether a specific long term effect is there or not. There is no serious attempt at the validation of this model. There is no element of open loop adaptive control, that is "learning", in the model, let alone closed loop adaptive control, that is "experiment". It thus takes a rather poor view of our intelligence and adaptability. There is no attempt to learn from experience in the modelling of complex systems in economics, with the sterility of theoretical growth models which use only "stylized facts" and not empirical data, nor from the many pitfalls in econo-

metrics. Even here decision models are only now beginning to be explored.

Professor Meadows is disarmingly ready to admit the imperfections of his model, claiming only that it is better than all other world models available today. Since decisions in conflict with the results of his model simulations are being made by policy makers in the light of inferior models, mental or written, Professor Meadows felt obliged to publish this popular account for a wider community than scientists, before scientists, social as well as physical, have seen a technical description of the work. The fallacy in this position is to suppose that the decision process in human society is the result of a single model in the mind of any one man, or system, or organization. The decision process is the result of feedback loops with many models constantly modified by experience of many kinds, the performance of the whole being far beyond the capacity of any part because of the manifold interactions and feedbacks from experience. This is why the world has been able to get by so far without Professor Meadows's model. The overall models of man and nature that have been used, the political, religious, and philosophical systems of mankind, have dealt with a rather wider range of phenomena. There may well be a role for simple global material flow models, even of the simplicity of that in this book, but to have any practical implications for policy they will have to be more firmly based in reality, and better linked with the more complex systems behaviour models which man has found necessary throughout history¹.

JEREMY BRAY

¹ Bray, J., *The Politics of the Environment* (Fabian Society, London, 1972).

Chinese Directory

Directory of Selected Scientific Institutions in Mainland China. Prepared by Surveys and Research Corporation. (Hoover Institution Series 96.) Pp. xxii + 469. (Hoover Institution: Stanford, California, June 1971.) \$19.50.

THE aim of this directory is to provide, for the first time, detailed information on nearly 500 of the more prominent institutions of the People's Republic of China that are "engaged in scientific research and other activities". It covers the basic fields of physical, biological, medical, agricultural and engineering sciences. A concise introduction describes the organization and management of science and technology in China followed by a short users guide. The directory proper is grouped under five broad sections: the academies, other government agencies,

industrial enterprises, professional societies, universities and specialized colleges. The information provided for each of the 490 institutions is listed under the following headings: name, location, personnel, structure, affiliations, research and other activities and biographical information. In addition three appendices follow the main section; a Pin Yin/Wade-Giles conversion table, the names of an additional 1,127 identified research and development institutes about which little or no information is available, and a set of maps to show the spatial distribution of the 490 institutions. A comprehensive set of indexes makes the directory easy to use for quick reference.

The compilers make no claim that the 490 institutions are representative of the whole of the Research and Development sector. As is to be expected, no information is available on the large national defence research sector. But it is reasonable to suppose that most of the other significant research institutions have been included in the main section of the directory. The termination date for the study was spring 1967, so none of the many institutional changes that followed the "cultural revolution" have been included. This book is nonetheless still valuable in that it gives the first comprehensive detailed information on research institutions up to 1967 and will be indispensable for seeing what changes have taken place when more new information becomes available.

R. J. CONROY

Resources of the USSR

Natural Resources of the Soviet Union: Their Use and Renewal. Edited by I. P. Gerasimov, D. L. Armand, and K. M. Yefron. Translated from the Russian by Jacek I. Roamnowski. English edition edited by W. A. Douglas Jackson. Pp. xiii+349. (W. H. Freeman and Co.: San Francisco and Reading, October 1971.) £6.

THIS important collection of essays, produced under the direction of the Institute of Geography of the Academy of Sciences of the USSR, reflects the concern of Soviet geographers with problems of conservation. Unlike many of their Western colleagues, geographers in the USSR seem to have no doubts about the peculiar value of their discipline in the consideration of such problems. "... The conservation of nature," they write in the preface, "is complex geographically, requires an assessment of the natural conditions of every region, and can be successful only if geographical methods are employed. This latter requirement has not always been stressed sufficiently".

In a day of increasing concern with the preservation of the natural environment, no excuse is needed for the publication of a volume of this type. My major criticism is that this well-written and well-produced work, replete with most valuable statistics and diagrams, was not republished in the West much earlier. The book appeared in Moscow as long ago as 1963, and hence sections are now somewhat dated. Nevertheless, a set of useful and informative notes by the editor in part closes this gap, and in particular rectifies economic predictions made in the text in the light of subsequent performance.

Much interest naturally attaches to the fact that these essays, often highly critical of previous planning failures, indicate that the centralized economy of the USSR faces the same conservation difficulties as the more diversified economies of the West. Central planning is not always sensible planning. It is, however, entirely characteristic that the writers should believe that only their economic system can solve such problems. "The USSR has the necessary political and economic prerequisites for planning the use and renewal of natural resources: national ownership of land, a planned economy, a unified economic policy. . . ." But in practice the consequences have often been the opposite to those intended. "Nature is often regarded as an inexhaustible reserve, and the only concern is how fast the resource can be exploited."

Most aspects of the natural environment are treated here, the principal gap being minerals, in the USSR the prerogative of the geologist. Because the essays were intended for the specialist, they are frequently technical, but closely allied to specifically Soviet situations. The grandiose scheme—the planting of shelter belts on a huge scale, the diversion of rivers—still plays largely upon the Soviet imagination. But there is evidence here that they have learnt from previous errors—the falling level of the Caspian and Aral Seas, caused respectively by hydroelectric schemes on the Volga and by the expansion of the irrigation network, the reduction in fauna resulting from indiscriminate forestry and the Virgin Lands Scheme, are all graphically described. More importantly the volume indicates a continuing preoccupation with the backward state of agriculture. Special emphasis is laid on land improvement schemes, schemes which outlived Khrushchev and continue in the 1970s.

The translation is good, the footnotes, references and index are all most full. The occasional printing error does little to detract from what is in effect a most useful addition to the available literature in English on aspects of Soviet geography.

DENIS J. B. SHAW

Genetic Variation

Genetic Diversity and Natural Selection. By K. James Murray. Pp. viii+128. (Oliver and Boyd: Edinburgh, February 1972.) £2.50.

PROFESSOR MURRAY teaches at the University of Virginia and is best known for his work with B. C. Clarke on the ecological genetics of *Partula* species. His purpose in writing the book under review was "to attempt an assessment of the present state of knowledge of variation in populations and to inquire whether the concept of natural selection may not be of crucial importance in our understanding of the situation"; his hope is that it "will provide an introduction to the literature of ecological genetics and evolution for advanced undergraduate and beginning graduate students". He is probably successful in this, although students will need a fair grounding in genetics if they are to make full use of what Professor Murray has to tell them. Moreover, they must not be allowed to gain all their understanding of the maintenance of genetical variation from this work: one of its main aims is to argue the importance of frequency-dependent selection in a wide range of ecological situations rather than to present a review of modern doctrine. This is legitimate, but it has to be interpreted in the context of the weight of dogma that population genetics has inherited from its theoretically-minded proponents. This often makes the separation of fact from interpretation as elusive as in some religious traditions.

The book has five main chapters: polymorphism, frequency-dependent selection, the evolution of dominance, the Wallace effect (hybrid infertility, not zoogeography), and parapatric speciation. Each provides an adequate survey of its subject within the terms of reference that Murray gives himself. Indeed I was pleased to learn of several pieces of work in a field in which he considers himself reasonably at home. In brief, a lucid synthesis of a variety of genetical and ecological concepts; worthwhile reading for any concerned with the study of populations; useful background for mathematicians, biochemists, and others unfamiliar with the complexities of real populations. Moreover, since one of the most relevant tasks facing biologists is, in Murray's words, "the specification of the components of fitness affected by heterotic selection . . . (which) can only be met by a joint attack combining the resources of protein chemists and ecological geneticists working together on the functional properties of enzymes and on the distribution of alleles in space and time", it is to be hoped that the readership will not be confined to converted ecologists.

R. J. BERRY

Tectonics

The Dynamic Earth. By Peter J. Wyllie. Pp. xiv+416. (Wiley: New York and London, January 1972.) £8.20.

THE showing on BBC Television recently of a programme on "The Restless Earth" has drawn public attention in Britain to the exciting story of the revolution in the sciences of the solid Earth which has occurred during the past five years. Professor Wyllie was organizing at the University of Chicago during this period a first year graduate course aimed at giving students "a bird's eye review of geology, and a global geophysical framework within which they could locate their specialized research topics in mineralogy, petrology, or geochemistry". The book under review is based on this course and incorporates much material and many ideas generated by the new global tectonics up to the autumn of 1970, by which time the International Upper Mantle Project (which had stimulated so much interest in global problems) was about to be succeeded by the International Geodynamics Project. The volume and complexity of the intricately related phenomena (physical, chemical and biological) which are the subjects of study in the Earth sciences are such that it is still far too early to look for a completely unified and authoritative treatment of even one branch of the science (such as the physico-chemical aspects of the Earth's development). Professor Wyllie has successfully dealt with this problem by presenting synopses of the data and ideas of key protagonists in the developing story of the new tectonics. This gives the book considerable value in a period of rapid change. As would be expected from an author whose own research interests lie especially in experimental petrology, the book is weightier in the petrological fields to which four out of the 15 chapters are devoted. The chapter on magma generation is thought-provoking. The emphases of the book make it particularly useful to those who are not petrologists, and especially to solid earth geophysicists.

S. A. F. MURRELL

Quantum Chemistry

Semi-empirical Self-consistent - field Molecular-orbital Theory of Molecules. By J. N. Murrell and A. J. Harget. Pp. x+180. (Wiley: New York and London, January 1972.) £4.50.

THE purpose of this book is to introduce the non-specialist to recent semi-empirical methods of calculating molecular wave functions and properties. It is

addressed primarily to the experimentalist who wishes to employ quantum mechanics as an interpretive and predictive tool.

The authors aim to guide the consumer through the many products available (ZDO-SCF, CNDO, MINDO, and so on) and to give him a general awareness of underlying principles and approximations. This they do with considerable success. Wisely, they have avoided the *ab initio* methods which require the most powerful computing facilities and the most discriminating application: experimental chemistry is usually concerned more with concepts than with precise numerical values, and the simple schemes developed by Hückel, Coulson and Longuet-Higgins, Dewar, Pople, and others still serve to introduce such concepts and give them semi-quantitative substance, with a minimum of mathematical analysis and arithmetic effort.

After a brief review of Hückel theory and its redevelopment in semi-empirical self-consistent form, using a zero differential overlap approximation, the authors give a long and useful survey of the essentially similar approximation methods now widely used in all-valence-electron calculations. The literature is well covered and much attention is given to the many prescriptions for choosing parameter values. The remainder of the book (more than half) is concerned with applications to chemical reactivity and related topics, and to the general area of magnetic resonance spectroscopy—a welcome shift of emphasis from valence theory *per se*.

It is easy to find fault with a book which aims to present difficult and often controversial material, from a broad and developing area, to a wide and non-specialist readership: in some places the text inevitably reads like an annual report, lacking in critical appraisal; in others, oversimplified or fallacious arguments are presented without qualification, and jargon without explanation. But in spite of such blemishes the book succeeds in providing a compact introduction to semi-empirical methods. It should be prescribed reading for any chemist who wishes to operate quantum chemistry computer programs with a reasonable degree of understanding, and is a welcome addition to the literature.

R. MCWEENY

Preservation of Art

The Conservation of Antiquities and Works of Art: Treatment, Repair and Restoration. By H. J. Plenderleith and A. E. A. Werner. Second edition. Pp. xix+394+46 plates. (Oxford University: New York and London, March 1972.) £6.50.

THE first edition of this book appeared in 1956 under the authorship of Dr Plenderleith. In this revision he has been joined by Dr Werner, the present Keeper of the British Museum Research Laboratory and his successor to that post. The book therefore represents the code of practice of that laboratory and the choice of methods for inclusion—and examples of their use—reflects this.

As with the previous edition, the conservation of most materials used in antiquity, and in works of art, is dealt with in three main sections: "Organic Materials"; "Metals"; "Siliceous and Related Materials". There are some omissions. For example there is no reference to the conservation and lifting of Roman pavements or the reconstruction of Roman wall plaster.

The need for this new edition is a consequence of the developing nature of the subject. But, although it is said in the preface that the book has been partly rewritten, much of the text is familiar. The overall length has increased from 375 to 394 pages. New plates have been added, an approximately equal number of old ones retained and the overall total reduced in number. With the changing value of money the price has nearly trebled.

The revision reflects the introduction of new materials used in consolidation, new methods (for example consolidative reduction in the treatment of corroded silver and lead, or benzotriazole in the treatment of bronze disease) and new items of equipment.

Lack of change in some sections underlines the well-established values of certain procedures. The section dealing with waterlogged wood, however, illustrates that there is still a need for improvement in some aspects of conservation. Thus, although the introduction of the polyethylene glycol method has solved some problems, that of how to deal with large timbers still exists and the authors are forced to recommend wrapping in damp sacking followed by prolonged slow drying.

One aspect which might be given more emphasis and development in future editions is that of the collection of the associated evidence preserved in the corrosion products of excavated objects (that is, textile, wood, leather, insect remains, and so on); an activity which must precede treatment of the corrosion products. Also the statement that "only in rare cases can radiographic methods of examination be considered essential" does less than justice to a much-used method of exploring objects before conservation.

But these are quibbles and do not detract from what was and will remain a solid work of reference which carries an impeccable stamp of authority as befits its authorship. JOHN MUSTY

CORRESPONDENCE

Insult to Medvedev

SIR,—Many of your readers will have learnt of the shabby treatment of Dr Zhores Medvedev, the distinguished Russian biologist, by the Soviet authorities. Dr Medvedev was invited by the International Organizing Committee to address the International Gerontological Congress in Kiev last week, but the local organizers of the Congress refused to allow him to participate in the meeting in a scientific capacity. It is now reported that some forcible means was then used to cut short a private visit that he made to Kiev in order to meet his many scientific colleagues from abroad.

This gross insult to the international community of science will surely not go unnoticed. But more effective than verbal protests would be a systematic scrutiny of the credentials of Russian visitors to international conferences and other scientific occasions. It is well known, for example, that the persons who eventually arrive at such meetings from the Soviet Union are not always those distinguished scholars who have been personally invited, or applicants whose scientific standing has been checked in advance by the convenors of the conference. The exclusion of the unknown and incompetent "substitutes" who sometimes turn up would do no harm to our own scientific activities nor to the legitimate interests of the Soviet scientific community. Out of extreme courtesy, this policy, whose propriety is beyond question, has seldom been followed. In view of this unprecedented discourtesy by the Soviet authorities, it should now be established as our regular custom.

Yours faithfully,

JOHN ZIMAN

University of Bristol,
H. H. Wills Physics Laboratory,
Royal Fort,
Tyndall Avenue,
Bristol BS8 1TL

Nuclear Uncertainty

SIR,—Your leader (*Nature*, **237**, 474; 1972) on the second report of the Select Committee on Science and Technology for 1971/72 understandably follows the precedent set by the Committee in distorting the CBI's views on Harwell by

selective quotation, but perhaps you will allow me to set the matter straight.

The CBI has at no time advocated that Harwell's non-nuclear work should be closed down, although when the Select Committee asked what the effect on industry of such a hypothetical closure would be, the reply was as quoted. Obviously such a closure would cause disruption if implemented suddenly, but the demands for the non-nuclear services at present supplied by Harwell to industry could eventually be satisfied by other laboratories if adequate notice were given.

Returning to the real world, however, the CBI view of publicly owned laboratories (including Harwell) is that they should work primarily for the Government or its agencies, but that their special skills and facilities should be made available to other users at properly costed rates. The CBI thinks that such contract work should form only a small part of the work of any Government laboratory, but is content to let the market decide the exact size of the activity, provided that the charges are an accurate reflexion of the cost of providing the service.

The only aspect of Harwell's work which the CBI suggested to the Select Committee was in need of special control was its speculative innovatory work described as "development of commercial agreements". The Select Committee appears to have taken this point, but in place of the CBI's proposed limitation of spending on such work to a fixed proportion of Harwell's expenditure, the Committee suggests supervision by an Industrial Advisory Committee which would presumably be held accountable in some way for the results of their advice.

It is not clear in what sense Harwell "pioneered industrial contract research" (IRD at Newcastle and others would no doubt have views on this matter), but the speed with which you pass from this statement to a discussion of Harwell's contract income tends to conceal the fact that about half this income comes not from industry but from Government departments. This source seems to the CBI to be entirely appropriate, and the proportion of income derived from it will no doubt increase in the post-Rothschild era, but this income is not industrial contract income. Whether Harwell will or will

not achieve its projected income from industry of about £3.5 million by 1974/5 remains to be seen, but the total R and D expenditure by industry which was put out to other sectors in 1968/69 (the latest year for which figures are available, see *Economic Trends*, No. 205, ix, November 1970) was only £9 million.

Yours faithfully,

D. W. BUDWORTH

21 Tothill Street,
London SW1H 9LP

Population Growth

SIR,—A brief comment on your defence of the "backlash" against Dr Ehrlich and others (*Nature*, **237**, 360; 1972). The whole point about exponential growth and the factors which "cause growth rates (to) change in the course of time under the pressure of events" is contained in the last phrase of that quotation. It is all very well to observe populations failing to continue to expand indefinitely, but when one happens to be a member of the species concerned the "pressure of events" can, I am sure, be excessively unpleasant. It is precisely to avoid being subject to such environmental pressures (which in many ecological cases cause an eventual plunge, rather than merely a levelling-off, in population, and may be expected to do so especially in our case since we have the means artificially to stave off those pressures for a time) that conservationists are urging that voluntary steps be taken not to delay the onset of natural restraints a little (as the technologists can) but to contain man's population and activities within what his natural environment can indefinitely support. Certainly we need governmental and international machinery to ensure that these steps are taken, but, since much of the resistance to expansion comes from the vested interest of industrialists and economists, often intimately concerned in government, this is not so robust a goal as your unconvincingly optimistic article suggests.

Yours faithfully,

ANTHONY WREN

26 Rosemont Court,
Rosemont Road,
Acton,
London W3 9LS

Organochlorine Propaganda

SIR,—Some say that propaganda induces people to believe statements that are not rationally convincing. A. H. Macpherson (*Nature*, 237, 413; 1972) writes that "persistent pesticides are believed" to be responsible for the nearly complete disappearance of the peregrine falcon. This is the theme of much propaganda; certainly many people believe it, but others do not. For example, the Wilson Committee¹ regarded the evidence as not precise enough to establish the postulated causal relationship. The Neuberger Committee² adopted an agnostic attitude. The Mrak Committee³ doubted if the organochlorine insecticides occur

naturally at sufficient levels to produce effects on reproductive success and population numbers.

We have been urged to accept the belief that organochlorine insecticides are the cause of population declines in predatory birds partly because no other acceptable cause has been proposed. On the contrary, the drastic decline in numbers of almost all birds of prey in the 19th and early 20th centuries has been attributed to the increased efficiency of firearms and the spread of game preservation. According to Cramp⁴, reduced activity of gamekeepers accounted for an improvement in populations after that and until the 1950s. Barber⁵ suggested that there has, in the 1960s, been an increase in

gamekeeping and considered that attacks on predatory birds with trap, poison and gun are a feature of this period. We must not jump to conclusions about this, but clearly the pesticide hypothesis is not the only one.

D. L. GUNN

Chilham, Kent

¹ *Advisory Committee on Pesticides and other Toxic Chemicals*, 148 (HMSO, 1969).

² *Third Report of the Research Committee on Toxic Chemicals*, 69 (Agricultural Research Council, 1970).

³ *Report of the Secretary's Commission on Pesticides and their Relationship to Environmental Health*, 677 (US Department of Health, Education and Welfare, 1969).

⁴ Cramp, S., *British Birds*, 56, 124 (1963).

⁵ Barber, D., *New Scientist*, 54, 212 (1972).

Obituary

Dr W. R. Thompson

DR WILLIAM ROBIN THOMPSON, FRS, former Director of the Commonwealth Institute of Biological Control, died on January 30, 1972. With his death entomology in general and biological control in particular have suffered a severe loss.

W. R. Thompson was born in London, Ontario, on June 29, 1887, the son of the late William Thompson, editor of the *Canadian Agricultural Weekly*. Educated at the Universities of Toronto and Cornell, where he obtained his BSc and MSc respectively, Thompson went on to do research at Cambridge between 1914 and 1915, a DSc at Paris in 1921 and a PhD at the University of St Maximin in 1924. During this lengthy period of university education from 1909 to 1919, Thompson also worked for the United States Department of Agriculture as an entomologist for four years, and for the Royal Navy as a bacteriologist from 1915 to 1919. In 1919 he again returned to work for the USDA where he undertook extensive work on the natural enemies of the gypsy moth and the European corn-borer, basing his work on the principles of biological control.

In 1928 Thompson was appointed assistant director of the Imperial Institute of Entomology, and in 1947 he became director of the Imperial Parasite Service which subsequently became the Commonwealth Institute of Biological Control, a post which he held until his retirement in 1958.

Thompson's career in entomology and biological control was long and distinguished. He was one of the pioneers of the scientific study of the subject and

he also studied the mathematical theory of population growth in connexion with parasite-host relationships and its bearing on population control. He also did considerable research on the taxonomy and systematics of tachinids, work he continued after his retirement.

While assistant director of the Imperial Institute of Entomology and director of the CIBC, Thompson was responsible for the extensive development of the control work in the institutes. In the early years much of it was carried out in Europe for Canada, New Zealand and Australia, but later, with the establishment of research stations in the West Indies, India, Pakistan, Argentina and California, similar work was possible on a much broader basis.

Many honours were accorded Thompson during his working life. He was made a Fellow of the Royal Society in 1933 and of the Canadian Royal Society in 1949. From 1947 to 1958 he edited the *Canadian Entomologist* and in 1956 he was President of the 10th International Congress of Entomology. He was a honorary fellow of, among others, the Royal Irish Academy and the Royal Entomological Society. He won many awards and medals as tributes to his work and was a honorary DSc of the Universities of Bordeaux and Carleton.

As well as publishing numerous articles and papers and a book (*Science and Common Sense*, 1937), Thompson was also responsible for the *Catalogue of the Parasites and Predators of Insect Pests*, an invaluable reference work to the literature of biological control.

With his death entomology has lost a personality whose impact in the field of biological control will be of lasting academic and economic value.

Dr Vilmos Várterész



DR VILMOS VÁRTERÉSZ, director of the Frédéric Joliot-Curie National Research Institute for Radiobiology and Radiohygiene, Budapest, Hungary, died after a brief illness on May 24, 1972.

Dr. Vilmos Várterész was born on August 9, 1917. He took his doctor's degree at the Kossuth Lajos University for Medicine in Debrecen. In 1942, as a young physician he started working in the Lóránd Eötvös Radium and X-Ray Institute, Budapest. There, for more than eleven years, he was engaged in the various fields of tumour therapy, in particular studying the biological effects of ionizing radiations, a subject that became the focus of his life's work. In 1957, as an already acknowledged expert of the field, at the request of the Hungarian Ministry of Health, he founded the Institute for Radiobiology of which he became the direc-

tor. Now began the most creative era of his scientific career during which he produced 145 articles and his book *Radiobiology* which was published both in Hungarian and German.

In 1959, in recognition of his scientific achievements, he became a candidate member of the Hungarian Academy of Sciences. With the advancing years he participated in the work of more and more national and international associations and organizations. To mention only the most important, he became the Vice-President of the Joint Committee for the Medical Application of Isotopes of the Hungarian Academy of Sciences and Ministry of Health, a member of the presidium of the National Atomic Energy Committee of Hungary, of the Hungarian Society for Nuclear Medicine, the Secretary for Hungary of the European Society for Radiation Biology, of the National Committee of the International Radiation Production Association, the member of the Expert Advisory Panel on Radiation of the World Health Organization.

His premature death in the prime of his life is a grievous loss both to the institute and to radiobiology.

Announcements

University News

Dr A. J. Ellison, Queen Mary College, London, has been appointed professor and head of the Department of Electrical and Electronic Engineering, City University.

Professor Bruno A. Boley, Cornell University, has been appointed dean of the Technological Institute of the Northwestern University.

The personal title of professor of chemistry has been conferred on **Dr J. E. Prue**, reader in the Department of Chemistry, University of Reading.

Appointments

R. A. Peddie, a member of the Central Electricity Generating Board, has been appointed part-time member of the UK Atomic Energy Authority.

Mr R. St J. Walker has been appointed secretary of the Science Research Council in succession to **Dr W. L. Francis**.

Miscellaneous

The Experimental Pathology Club Prize has been awarded to **Dr A. E. Butterworth**, University of Cambridge, for his essay "Cytotoxic Effects of Antigen-Transformed Lymphocytes *in vitro*".

The Chalmers medal of the Royal Society of Tropical Medicine and Hygiene has been presented to **Dr W. W. Macdonald** of the Liverpool School of Tropical Medicine.

Mr F. P. Thomson is preparing a biography of the late **Alan Dower Blumlein** who was chief engineer of the EMI Research Department. Anyone who knew Mr Blumlein or who would care to contribute an assessment of his position in the history of technology should write to Mr Thomson at 39 Church Road, Watford WD1 3PY, Hertfordshire.

Reports and Publications

not included in the Monthly Books Supplement

Great Britain and Ireland

The British Petroleum Company Limited. BP Statistical Review of the World Oil Industry, 1971. Pp. 24. (London: The British Petroleum Company Limited, 1972.) [15]

Furniture Development Council. 23rd Annual Report for the year ending 31 December 1971. Pp. 6. (Stevenage, Herts.: Furniture Development Council, 1972.) [15]

Advisory Committee on Oil Pollution of the Sea. Annual Report for 1971. Pp. 20. (Sandy, Beds.: Advisory Committee on Oil Pollution of the Sea, 1972.) [15]

The Heating and Ventilating Research Association. Annual Report for 1971. Pp. 32. (Bracknell, Berkshire: The Heating and Ventilating Research Association, 1972.) [15]

The Ciba Foundation for the Promotion of International Co-operation in Medical and Chemical Research. 1971 Report. Pp. 67. (London: The Ciba Foundation, 1972.) [25]

The Zoological Record. 1969. Vol. 106, Section 6: Vermes. Part A: Platyhelminthes, Nemathelminthes, Nemertinea, Mesozoa. Compiled by Eileen Mitchell, R. A. Bray and Stephen Prudhoe. Pp. vi + 148. £6.50; \$15.60. 1969. Vol. 106, Section 19: Mammalia. Compiled by the Staff of the Zoological Society of London. Pp. vi + 361. £12; \$29. (London: The Zoological Society of London, 1972.) [25]

List of British Palaeontologists. (Glasgow: The Palaeontological Association, Hunterian Museum, 1972.) [35]

Department of Trade and Industry. Patents in the Common Market. (Unofficial translation of a Second Preliminary Draft of a Convention for the European Patent for the Common Market and of First Preliminary Drafts of Implementing Regulations and of Rules Relating to Fees, prepared by the "Community Patent" Working Party.) Pp. 92. (London: HMSO, 1972.) 60p net. [35]

Innovation in Higher Education. Edited by Colin Flood Page and Harriet Greenaway. (Papers presented at the Seventh Annual Conference of the Society, December 1971.) Pp. 97. (London: Society for Research Into Higher Education, 20 Gower Street, WC1, 1972.) £1.60. [35]

Fisons Report and Accounts for 1971. Pp. 32. (London: Fisons Limited, 1972.) [35]

Index, Vol. 1, No. 1, Spring 1972. Edited by Michael Scammell. (A New Magazine on Censorship.) Pp. 1-164. Published quarterly. Subscriptions £2 for one year, £5 for three years. Students' rate £1.50 for one year. (London: Writers and Scholars International, 1972.) [45]

Greenwich Time Report, Royal Greenwich Observatory. Time and Latitude Service, 1971, April-June. Pp. 209-222. (London: Science Research Council, 1972.) [45]

Report of the Rugby School Natural History Society for the year 1971. Pp. 24 + 1 plate. (Rugby: Rugby School Natural History Society, 1972.) [55]

The Education and Training of Teachers—a Statement by the Council of the Institute on the James Report. Pp. 14. (London: University of London, Institute of Education, 1972.) [55]

Philosophical Transactions of the Royal Society of London. A: Mathematical and Physical Sciences. Vol. 272. No. 1221: A Discussion on Problems Associated with the Subsidence of Southeastern England. Organized by K. C. Dunham and D. A. Gray. Pp. 79-274 + plates 1-3. (London: The Royal Society, 1972.) £5; \$14. [55]

Report by the Hydrographer of the Navy for 1971. Pp. 36. (NP 130.) (Taunton, Somerset: Hydrographic Department, Ministry of Defence, 1972.) [85]

Department of Education and Science. Education and Science in 1971. Pp. 51. (London: HMSO, 1972.) 36p net. [85]

Foreign and Commonwealth Office—Overseas

Development Administration. Annual Report of the Directorate of Overseas Surveys for the year ended 31st March 1971. Pp. iv + 76 + 9 maps. (London: HMSO, 1972.) £1.35 net. [85]

Fabian Tract No. 413: Labour and the Economy—A Socialist Strategy. By Michael Stewart. Pp. 35. 35p. Young Fabian Pamphlet No. 30: British Entry—Labour's Nemesis. By James Bellini. Pp. 24. 25p. (London: Fabian Society, 1972.) [85]

The British Food Manufacturing Industries Research Association. Annual Report for 1971. Pp. 50. (Leatherhead: BFMIRA, 1972.) [85]

Ordnance Survey. Professional Papers, New Series, No. 23: The Development of Digital Maps. By Col. R. C. Gardiner-Hill, OBE. Pp. 14 + 7 figures. (Southampton: Ordnance Survey of Great Britain, 1972.) [85]

Language in Society, Vol. 1, No. 1, April 1972. Edited by Dell Hymes. Published twice a year. Pp. 1-176. Annual subscription £4; \$12. Single parts £2.50; \$7.50. (London and New York: Cambridge University Press, 1972.) [105]

University of Strathclyde. Annual Report 1970/1971. Pp. 132. (Glasgow: University of Strathclyde, 1972.) [105]

British Standards Institution. The Use of SI Units. Pp. 28. (London: British Standards Institution, 1972.) [125]

Building Research Station Digest, No. 141: Wind Environment Around Tall Buildings. Pp. 8. (London: HMSO, 1972.) 5p. [125]

Natural Environment Research Council. Institute of Hydrology—Research 1971-72. Pp. viii + 67. (Wallingford, Berkshire: Institute of Hydrology, 1972.) [125]

The Health Education Council. Report for the period 1 April 1970 to 31 December 1971. Pp. 26. (Wembley, Middx.: The Health Education Council, 1972.) [125]

Philosophical Transactions of the Royal Society of London. B: Biological Sciences. Vol. 264, No. 858: Biostratigraphical Dating of the Early History of the South Atlantic Ocean. By R. A. Reymont and E. A. Tait. Pp. 55-95 + plates 3-5. (London: The Royal Society, 1972.) £1.40; \$3.90. [125]

Other Countries

Smithsonian Year 1971: Annual Report of the Smithsonian Institution for the year ended 30 June 1971. Pp. vi + 247. (Washington, DC: Smithsonian Institution Press, 1971. For sale by US Government Printing Office.) \$1.25. [293]

Journal of Statistical Computation and Simulation, Vol. 1, No. 1, January 1972. Edited by Richard G. Krutchkoff. Subscription Rates (per volume postpaid), 4 issues per volume. Libraries: USA/elsewhere \$41.50/£17.30, Great Britain £15.85. Individuals who warrant the journal is for their own use and order direct from the publisher: USA/elsewhere \$14.50/£6, Great Britain £5.50. (New York, London and Paris: Gordon and Breach Science Publishers, 1972.) [293]

AMBIO, Vol. 1, No. 1, February 1972. (A journal of the Human Environment Research and Management.) Pp. 140. Published six times yearly. (Stockholm: AMBIO, Box 23136, S-10435, 1972. Subscriptions to Universitetsforlaget, PO Box 307, Blindern, Oslo, Norway.) USS13; Skr. 60, Nk and Dk 84. [293]

US Department of the Interior: Geological Survey. Water-Supply Paper 1991: Quality of Surface Waters of the United States, 1966. Parts 1 and 2: North Atlantic Slope Basins and South Atlantic Slope and Eastern Gulf of Mexico Basins. Pp. xvi + 984. \$4. Professional Paper 701-A: Hydrology of Two Small River Basins in Pennsylvania Before Urbanization. By R. Adam Miller, John Troxell and Luna B. Leopold. With a section on Observations of Stream Fauna by Ruth Patrick and Robert W. Grant, Jr. Pp. iv + 57. 75 cents. Professional Paper 732-A: Channel Movement of Meandering Indiana Streams. By James F. Daniel. Pp. iii + 18. 35 cents. (Washington, DC: Government Printing Office, 1971.) [293]

Handbook of Electroencephalography and Clinical Neurophysiology, Vol. 4, Part B. Pp. 64. (Amsterdam: Elsevier Publishing Company, 1972.) [303]

Towards Self-Reliance in Agriculture. (IARI Research Bulletin, New Series, No. 7.) Pp. 82. (New Delhi: Indian Agricultural Research Institute, 1972.) [44]

Pharmaceutical Manufacturers Association. Annual Survey Report 1970/1971. Pp. 20. (Washington, DC: Pharmaceutical Manufacturers Association, 1972.) [44]

Canada Department of Agriculture: Research Branch. Monograph No. 6: Arctic Adaptations in Plants. By D. B. O. Savile. Pp. 81. (Ottawa: Information Canada, 1972.) [44]

US Department of the Interior: Geological Survey. Water-Supply Paper 1999-C: Water Resources of the Upper White River Basin, East Central Indiana. By L. W. Cable, J. F. Daniel, R. J. Wolf and C. H. Tate. Pp. v + 38 + 4 plates. (Washington, DC: Government Printing Office, 1971.) [54]

US Department of the Interior: Geological Survey. Water-Supply Paper 1532-D: Effects of Grazing on the Hydrology and Biology of the Badger Wash Basin in Western Colorado, 1953-66. By G. C. Lusby, V. H. Reid and O. D. Knipe. Pp. v + 90 + 1 plate. \$1. Professional Paper 683-D: Climatic Implication of a Late Pleistocene Ostracod Assemblage from Southeastern Virginia. By P. V. Valentine. Pp. iii + 28 + 4 plates. 70 cents. (Washington, DC: Government Printing Office, 1971.) [64]

Bulletin of the American Museum of Natural History. Vol. 147, Article 3: Comparative Myology

of Jaw, Hyoid, and Pectoral Appendicular Regions of New and Old World Hystricomorph Rodents. By C. A. Woods. Pp. 115-198. \$2.85. Vol. 147, Article 4: Taxonomy and Distribution of the Black Fly Subgenus *Simulium* (*Pternispatha*) Enderlein (Simuliidae, Diptera, Insecta). By S. Coscaron and P. Wygodinsky. Pp. 199-240. \$1.95. (New York: American Museum of Natural History, 1972.) [64]

Smithsonian Contributions to Zoology. No. 76: Proceedings of the First International Conference on Meiofauna. Edited by N. C. Hurlings. Pp. viii + 205. \$2.25. No. 98: The Shrimps of the Smithsonian-Bredin Caribbean Expeditions with a Summary of the West Indian Shallow-water Species (Crustacea: Decapoda: Natantia). By F. A. Chace, Jr. Pp. x + 179. \$2. No. 112: Mimetic Relationships Involving Fishes of the Family Blenniidae. By V. G. Springer and W. F. Smith-Vaniz. Pp. 36 (7 plates). 50 cents. (Washington, DC: Smithsonian Institution Press, 1972. For sale by US Government Printing Office.) [64]

A Directory of Standards Laboratories, 1971 edition. Pp. 42. (Gaithersburg, Md: National Conference of Standards Laboratories, 1971.) [64]

Environment and Development: The Founex Report on Development and Environment, with commentaries by Miguel Osozorio de Almeida, Wilfried Beckerman, Ignacy Sachs, and Gamani Corea. Pp. 84. (New York: Carnegie Endowment for International Peace, 1972.) \$1. [74]

Research Council of Alberta. Annual Report 1971. Pp. 79. (Edmonton: Research Council of Alberta, 1972.) [74]

US Department of the Interior: Geological Survey. Professional Paper 627-F: Summary of the Hydrological Situation on Long Island, New York, as a Guide to Water-Management Alternatives. By O. L. Franke and N. E. McClymonds. Pp. v + 59. (Washington, DC: Government Printing Office, 1972.) 65 cents. [74]

Institut Royal des Sciences Naturelles de Belgique. Mémoires. No. 164: Recherches sur l'Ecosystème Forêt. Biomasse, Productivité Cycle des Polyéléments Biogènes dans l'Ecosystème "Chêne à Caducifoliée"—Essai de Phytochimie Forestière. Par P. Duvinageau, S. Denayer, P. Ambroes et J. Timperman. Pp. 101. No. 165: Ecologie et Biogéographie de Quelques Peuplements d'Arthropodes Edaphiques. Par P. Lebrun. Pp. 203. No. 166: *Weichsella reticulata* (Stokes et Webb) Fontaine from the Wealden of Belgium. By Kenneth L. Alvin. Pp. 33 + 9 plates. (Bruxelles: Institut Royal des Sciences Naturelles de Belgique, 1971.) [74]

Arteriosclerosis: a Report by the National Heart and Lung Institute Task Force on Arteriosclerosis. Vol. 1. (DHEW Publication No. (NIH) 72-137.) Pp. 41. (Washington, DC: Government Printing Office, 1971.) 50 cents. [104]

Nederlandse Vereniging voor Sterrenkunde. Observations of Variable Stars, July-December 1971. (Report No. 21.) Pp. 11. (Groningen, Netherlands: Kapteyn Astronomical Laboratory, 1972.) [104]

US Department of the Interior: Geological Survey. Bulletin 1354-E: Age and Correlation of the Clinchfield Sand in Georgia. By S. M. Herrick. Pp. 17. 15 cents. Techniques of Water-Resources Investigations of the United States Geological Survey. Chapter C3: Computation of Fluvial-Sediment Discharge. Book 3: Applications of Hydraulics. By George Porterfield. Pp. vi + 66. 75 cents. (Washington, DC: Government Printing Office, 1972.) [104]

The Arms Race: Steps Toward Restraint. By Herbert Scoville, Jr., Betty G. Lall, and Robert E. Hunter. (International Conciliation.) Pp. 58. (New York: Carnegie Endowment for International Peace, 1972.) [104]

Fondation Universitaire. 51e Rapport Annuel, 1970/1971. Pp. 149. (Bruxelles: Fondation Universitaire, 1972.) [104]

Institut Royal des Sciences Naturelles de Belgique. Mémoires. No. 167: Recherche sur la Transmission des Caractères Mesurables de l'Homme. Par Charles Susanne. Pp. 147. No. 168: Les Epiphytes des

Feuilles de *Posidonia oceanica* Delile sur les Côtes Françaises de la Méditerranée. Par Dick van der Ben. Pp. 100. No. 169: Two Toxochelyid Sea Turtles from the Landenian Sands of Erquennes (Hainaut) of Belgium. By R. Zangerl. Pp. 32 + 9 plates. (Bruxelles: Institut Royal des Sciences Naturelles de Belgique, 1971.) [104]

Institut Royal des Sciences Naturelles de Belgique. Mémoires. Deuxième Série. Fasc. 85: A Catalogue of Living and Fossil Cowries. Taxonomy and Bibliography of Triviacea and Cypraeaacea (Gastropoda Prosobranchia). By Maria Schilder and Franz Alfred Schilder. Pp. 246. Fasc. 86: Amazonian Algae. By K. Thomasson. Pp. 57 + 24 plates. (Bruxelles: Institut Royal des Sciences Naturelles de Belgique, 1971.) [114]

Sweden. Royal Ministry of Foreign Affairs. Royal Ministry of Agriculture. Air Pollution Across National Boundaries: The Impact on the Environment of Sulfur in Air and Precipitation—Sweden's Case Study for the United Nations Conference on the Human Environment. Pp. 96. (Stockholm: University of Stockholm, Institute of Meteorology, 1971.) [124]

US Department of the Interior: Geological Survey. Bulletin 1325: Mineral Resources of the Pasayten Wilderness Area, Washington. By Mortimer H. Staatz et al. Pp. xv + 255 + 3 plates. Bulletin 1353-A: Mineral Resources of the Wilson Mountains Primitive Area, Colorado. By C. S. Bromfield. Pp. vii + 79. Water-Supply Paper 1980: Ground-Water Levels in the United States, 1966-70. Northeastern States. Pp. v + 228. \$1. (Washington, DC: Government Printing Office, 1971 and 1972.) [124]

Przegląd Bibliograficzny Opublikowanych prac Autorów Polskich na Temat Alkaloidów, Tom 1. Lata 1945-1964. By E. Lemanczyk. Pp. 130. (Poznań: Instytut Przemysłu Zielarskiego, 1971.) [134]

Zenith Radio Corporation. Annual Report for 1971. Pp. 26. (Chicago: Zenith Radio Corporation, 1972.) [134]

Canada: Department of Energy, Mines and Resources. Paper 71-2: Age Determinations and Geological Studies: K-Ar Isotopic Ages, Report 10. By R. K. Wanless, R. D. Stevens, G. R. Lachance and R. N. Delabio. Pp. vi + 96. \$2. Paper 71-19: Geological Map and Notes on the Ordovician and Silurian Litho- and Biostratigraphy, Anticosti Island, Quebec. By Thomas E. Bolton. Pp. v + 44 (12 plates). \$2. (Ottawa: Information Canada, 1972.) [134]

Central Mining Research Station, Dhanbad, India. Annual Report 1970-1971. Pp. 273. (Dhanbad, Bihar: Central Mining Research Station, 1972.) [134]

Berichte des Deutschen Wetterdienstes. Nr. 126: Über ein Neues Modell des Strahlungsbilanzmessers nach Schulze. Von Claus U. Däke. Pp. 22. (Offenbach a.M.: Selbstverlag des Deutschen Wetterdienstes, 1972.) [134]

Preventive Medicine, Vol. 1, Nos. 1-2, March 1972. An International Journal devoted to Practice and Theory. (Official Journal of the American Health Foundation.) Pp. 1-286. Volume 1 (4 issues) 1972. Price for institutional subscribers \$35. (New York and London: Academic Press, 1972.) [144]

Report of the King Institute of Preventive Medicine for the period 1st April 1966 to 31st March 1967. Pp. 7. Report of the King Institute of Preventive Medicine for the period 1st April 1967 to 31st March 1968. Pp. 28. (Madras: King Institute, 1971.) [144]

The Bee Language Controversy: an Experience in Science. By Adrian M. Wenner. Pp. 109. (Boulder, Colorado: Educational Programs Improvement Corporation, 1971.) \$3.95 hardback; \$1.95 paperback. [174]

Publications of the Finnish Geodetic Institute. No. 71: Astronomical Determinations of Latitude and Longitude in 1961-1966. By E. Kääriäinen. Pp. 102. No. 72: Plate Reduction for the Stellar Triangulation. By J. Kakkuri. Pp. 42. (Helsinki: Geodeettinen Laitos, 1971 and 1972.) [184]

Antarctic Map Folio Series. Folio 14: Birds of the Antarctic and Subantarctic. By G. E. Watson,

J. P. Angle, P. C. Harper, M. A. Bridge, R. P. Schlatter, W. L. N. Tickell, J. C. Boyd and M. M. Boyd. Pp. 18 + 15 plates. \$10. Folio 15: Coastal and Deep-Water Benthic Fishes of the Antarctic. By Hugh D. DeWitt. Pp. 10 + 5 plates. \$6. (New York: American Geographical Society, 1971.) [184]

Honeywell—1971 Annual Report. Pp. 46. (Minneapolis, Minnesota: Honeywell, Inc., 1972.) [194]

Nova Scotia Research Foundation. Selected Bibliography on Algae. No. 12, 1971. Pp. 172. (Dartmouth, Nova Scotia: Nova Scotia Research Foundation, 1972.) [204]

Canada: Department of Energy, Mines and Resources. Paper 71-50: Analysis of Rocks and Minerals by Atomic Absorption and Flame Emission Spectroscopy. Part 4: A Composite Scheme for the Less Common Alkali and Alkaline-Earth Elements. By Sydney Abbey. Pp. v + 18. 75 cents. Geological Maps. Map 1305A: Cape St. John's, District of Franklin. Map 1310A: Buxton Fiord, District of Franklin. (Ottawa: Information Canada, 1972.) [204]

Smithsonian Contributions to Zoology. No. 117: The Subgenera of the Crayfish Genus *Procambarus* (Decapoda: Astacidae). By Horton H. Hobbs, Jr. Pp. 22. (Washington, DC: Smithsonian Institution Press, 1972. For sale by US Government Printing Office.) 35 cents. [204]

National Institute of Genetics, Japan. Annual Report for 1970. Pp. 126. (Misima, Sizuoka-ken: National Institute of Genetics, 1971.) [204]

US Department of the Interior: Geological Survey. Water-Supply Paper 1532-F: Appraisal of Stream Sedimentation in the Susquehanna River Basin. By Kenneth F. Williams and Lloyd A. Reed. Pp. iv + 24. (Washington, DC: Government Printing Office, 1972.) 25 cents. [204]

Records of the Dominion Museum, New Zealand. Vol. 7, No. 14: *Venusstifer bouniyeensis* (Powell) (Mollusca: Stifiliferidae). By F. M. Climo. Pp. 143-146. Vol. 7, No. 15: A New Species of *Paryphanta* (Mollusca: Paryphantidae) from South-West Fiordland, New Zealand. By F. M. Climo. Pp. 147-150. Vol. 7, No. 16: On the Proper Usage of the Name *Terebratulina hedleyi* Finlay, 1927, for *Cancellothyrus australis* Thomson, 1927 (Brachiopoda). By E. W. Dawson. Pp. 151-154. Vol. 7, No. 17: The Marine Mollusca of the Royal Society Expedition to Southern Chile, 1958-59. By R. K. Dell. Pp. 155-233. Vol. 7, No. 18: A New Cat Shark (Sclerorhinidae) from New Zealand. By S. Springer. Pp. 235-241. Dominion Museum Records in Ethnology, Vol. 2, No. 8: Composite Adze Hafts in New Zealand Prehistory. By I. W. Keyes. Pp. 83-96. Report of the Board of Trustees, National Art Gallery and Dominion Museum for the year ended 31 March 1971. Pp. 40. 25c. (Wellington, NZ: Dominion Museum, 1971.) [204]

Australian Journal of Zoology Supplementary Series. Supplement No. 9: A Revision of the Scarabaeine Dung Beetles of Australia. I. Tribe Onthophagini. By E. G. Matthews. Pp. 330. Supplement No. 10: Revision of the Genus *Pachyprosopis* Perkins (Apoidea: Colletidae). By Elizabeth M. Exley. Pp. 43. (East Melbourne: Editorial and Publications Section, Commonwealth Scientific and Industrial Research Organization, 1972.) [214]

Lövö Geomagnetic Observatory—Year Book 1970. Pp. 38. (Stockholm: Sveriges Geologiska Undersökning, 1972.) [214]

Australia: CSIRO. Division of Soils Technical Paper No. 12: Moisture Content of Soils for Making Saturation Extracts and the Effect of Grinding. By J. Loveday. Pp. 9. (Melbourne: CSIRO, 1972.) [214]

World Health Organization. International Histological Classification of Tumours. No. 5: Histological Typing of Odontogenic Tumours, Jaw Cysts, and Allied Lesions. By J. J. Pindborg and I. R. H. Kramer, in collaboration with H. Torloni. Pp. 45 + 150 colour transparencies. (Geneva: World Health Organization; London: HMSO, 1971.) 150 Sw. francs, £15, \$50; Book only, 36 Sw. francs, £3.60, \$12. [214]

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